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ASSOCIATION THEORY TO-DAY

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# ASSOCIATION THEORY TO-DAY

AN ESSAY IN SYSTEMATIC PSYCHOLOGY

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## PREFACE

This essay deals with the present-day outcome of the theory of association. My object is not to glorify the past nor to show that there is nothing new under the sun. Neither am I interested in resurrecting ideas that would better remain in their becoming graves. I regard it as important that modern ideas of associative connection and organization have come out of a long history, but my principal concern is with that edition of the general theory of association which is to-day actually functioning in experimental psychology. It seems to me that this theory deserves much more thought and elaboration than it has had. The hope of clearly conceived experimentation in the field of learning waits upon a critical and exhaustive consideration of what the theory of association ought to be for the psychologist of to-day.

E. S. R.

LYME, CONNECTICUT  
*August, 1931*



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ASSOCIATION THEORY TO-DAY





## Chapter I

### THE PRESENT POSITION OF THE THEORY OF ASSOCIATION

#### Importance of Association in American Psychology

A CONCEPTION of association, or integration, is a central element in the systematic psychology that has grown up in this country during the past forty years. There have been exceptions, of course, even among important writers. The Titchenerian movement, engrossed as it was with the texture of experience, made only incidental use of the principle of association. This is still the case in the work of Bentley, one of the few systematic writers clearly belonging within the Titchenerian tradition. McDougall, who might be included among American writers because of his vigorous participation in many of their arguments, also lies outside the main current so far as associationism is concerned. McDougallism is a modern nativism and could hardly be tolerant of a theory that attempts to explain so much in ontogenetic terms. But most of the writers who have lately played an important part in the development of a general point of view in American psychology are to be placed within a coherent tradition, a tradition which, though not always actually dominated by the theory of association, contains the theory as an important and conspicuous feature. In the writings of Angell, Thorndike, Woodworth, Watson, Carr, Kantor, and Hollingworth the principle of association

is given extensive employment.<sup>1</sup> For all of them psychology contains no more fascinating story than that which tells how the individual's chaotic activities get associated, integrated, or grouped in the course of his life history. There are, among these writers, both fundamental and superficial differences as to what get associated—whether ideas or neurones, experiences or movements. There are differences of varying importance in respect to the laws of manifestation of the larger principle. But with all of these differences the central emphasis remains upon the connections among psychological activities produced within the life history of the individual.

British thought, until well on into the nineteenth century, was strongly associational, but the associationism of our times is not to be understood as simply a continuation of the British tradition. The emphasis that our writers have put upon association requires for its explanation reasons why the conception has had a peculiar fitness for the American environment. Perhaps because the American public has had an eager faith in the practical advantages of learning, the American scholar has felt a great call to be useful. Our philosophy has tried several doctrines, but pragmatism has been its one whole-hearted theory. Our physics has been a little mathematical, but very experimental. Our psychology has had its doctrines of sensation and of instinct, but these have been minority matters as compared with association, which opens up the school room. We can see why, then, among the numerous conceptions that might have dominated psycho-

<sup>1</sup> The foremost American interpreter and defender of the historic theory of association is Professor Howard C. Warren (see especially *A History of the Association Psychology*, 1921). In the expression of his own system, however, Professor Warren is perhaps less dominated by the theory of association than are the majority of recent American writers. This may be due to the fact that his *Human Psychology* is written from an analytic and physiological point of view rather than from that genetic point of view so characteristic of recent American theory.

logical thought, association should have had a genuine appeal. The texture of experience is something about which nothing can be done. Innate capacities are merely the limits within which an energetic improver of humanity can work. But association strikes at the heart of education. It holds a promise that changes can be worked in human nature, because it is, in fact, a theory of such changes.

The thesis of this essay is not, however, the importance of the theory of association in the scientific psychology of this country. That importance will be accepted. What I am really concerned about is the nature of the doctrine of association in so far as it can be formulated in terms acceptable to present-day thought.

Some of the authors here regarded as associationists would probably be unwilling to accept membership in that ancient clan. Hollingworth, for instance, says that his system is not associationism, because it deals with connections between "primal events in nature" and not with connection between "mere ideas."<sup>2</sup> Others restrict their use of the term *association* to organizations of ideational processes, though they clearly apply the same fundamental principles more broadly in the name of learning or habit formation. Now it would be foolish to deny that the theory of association under British care put an unwarranted emphasis upon the intellectual functions and that the contentions of the theory became strongly colored by philosophical doctrines of mind to which, we, to-day, feel superior. Nevertheless, there are ample grounds for considering that our present conceptions of learning and habit formation are stages in the development of the same general theory as that with which British associationism was concerned. The narrowness of certain British writers in the application of the theory has been taken entirely too seriously. There is evidence in Hartley, Spencer, and Lewes

<sup>2</sup> *Psychology: Its Facts and Principles*, p. v.

as well as in thinkers of the ancient world that the theory of association is capable of standing up under all sorts of theories of mind. So more is to be gained from admitting the ancestry of contemporary theory than from claiming a sudden emergence from darkness into light.

When associationism is foresworn in these times it is usually because of the final solution that the theory is assumed to have decided upon for some fundamental problem. Of course, one has a right to be suspicious of final solutions, but these irrevocable decisions with which the theory is so often charged have been decisions of this or that expositor and are by no means inevitably attached to the central standpoint, which has gone through such an adventurous history. Indeed, there is scarcely an argument advanced against associationism that has not been put forward vigorously as part of that theory by one or more of its prominent advocates. If there are grounds for rejecting associationism altogether, then Watson and Hollingworth should be rejected along with Thomas Brown and the Millses. Such a wholesale excommunication has seldom been recommended. As we advance upon the individual issues of the theory of association, let us ask then not whether a theory of association is tenable, but rather, what kind of a theory of association is acceptable in contemporary psychology.

### The Basic Conception of Association

To begin with, it is desirable to attain a statement of association that will be generally suitable as a basis for further discussion. This purpose will demand that we avoid for the present such issues as involve the detailed, rather than the essential, nature of the conception. It will be well, for example, to postpone the question as to exactly what factors do get organized in association—whether ideas and ideas,

stimuli and responses, or neural pathways and neural pathways. The fact that very similar and often identical dynamic principles have been applied by writers who have been irreconcilable enemies on the issue as to what is associated should prove the secondary character of this problem. Something must be taken for granted in order that discussion can begin. By *association* we may mean simply *the establishment of functional relations among psychological activities and states in the course of individual experience*. Even if there are objections to the precise wording of this formulation, its general import is comprehensible.

### Is the Conditioned Reflex Conception an Alternative?

Although the theory of association is touched by a number of recent developments in psychology, nowhere do we find the theory so relevantly involved as it is in connection with the conception of the conditioned reflex. There are to-day many psychologists who stand in awe before the educated salivation of a Russian dog. Among those who have gone into the conditioned reflex matter most seriously there is a certain tendency to connect the principle involved with the older principle of association.<sup>3</sup> Nevertheless, Pavlov's work takes such prominence in their exposition that one is justified in assuming that the conditioned reflex is regarded as something more than a good illustration of psychological association. Indeed, there is strong reason to believe that some enthusiasts, including Pavlov himself, would regard all cases of psychological association as manifestations of the conditioning of reflexes.

The affection that is shown for the conditioned reflex seems

<sup>3</sup> As, for example, F. A. C. Perrin and D. B. Klein, *Psychology* (1926), Chapter III.

to be based upon certain positive claims in its favor and upon a number of objections to a doctrine of association to which the conception of the conditioned reflex is alleged or implied to be superior. So far as I know, the actual comparison of the two conceptions has never been made in any thorough fashion. For this reason I shall state some arguments against the older theory which, as a matter of fact, advocates of the conditioned reflex have not yet used. These arguments are more or less standard objections to the association theory, however, and we shall do well to get them out into the open in this connection.

First among the positive points may be placed the claim that the conditioned reflex is an actual neural mechanism and, therefore, has a high order of explanatory value. Closely related to this is the contention that all behavior is reflex at bottom—that training is simply a process through which the reflexes become organized. The conception of concatenated reflexes so frequently introduced a few years ago is a phase of these assumptions.

Some critics of the conditioned reflex theory have said that its weakness lies in its attempted reduction of all that is dearest in life to a simple neural mechanism.<sup>4</sup> Such criticism, of course, simply makes the advocates of the theory smack their lips. The telling criticism, however, is that which has come from those who are eager to secure neurological interpretations for a wide range of phenomena. The first of these criticisms arises out of the straightforward experimental fact that in certain cases where reflexes have apparently been conditioned, careful analysis has shown that the response is not the same old response functioning in connec-

<sup>1</sup> See William McDougall, *Outlines of Psychology* (1923), pp. 21-34. McDougall employs more than the sentimental argument against the reflex theory, but the emotional element is certainly strong in his writing on this question.

tion with a new type of stimulation, but that the new response is neurologically quite unlike the old response to which its superficial aspects bear a resemblance.<sup>5</sup> There is evidence, too, that one need not always go inside the nervous system to discover that the response, when it comes to function under new conditions of stimulation, may be radically different. Professor Dodge tells me that thus far he has been unable to secure a conditioned response which has approached identity with a reflex prototype. He has compared a conditioned knee-jerk with a knee-jerk in response to the originally adequate condition of stimulation. The muscular occurrence after conditioning was radically different from the original response particularly in respect to reaction time. These facts do not indicate that a response can never be kept in practically original form except for its connection with a new condition of stimulation. They do indicate that we are far from sure that complicated behavior is only a realignment of reflexes.<sup>6</sup>

The attempt to explain acquired behavior in terms of elaboration of basic reflexes suffers on another account besides the difficulty of proving the constancy of a given reaction

<sup>5</sup> J. M. Lang and J. M. D. Olmsted, "Conditioned Reflexes and Pathways in the Spinal Cord," *American Journal of Physiology* (1923), 65, 603-611. See also F. A. C. Perrin and D. B. Klein, *Psychology* (1926), pp. 104-109.

<sup>6</sup> Since this paragraph was written there have appeared two major studies of conditioning, both of which emphasized the complexity of this process. See G. R. Wendt, "An Analytical Study of the Conditioned Knee-jerk," *Archives of Psychology*, 1930, No. 123, and E. R. Hilgard, "Conditioned Eyelid Reactions to a Light Stimulus Based on the Reflex Wink to Sound," *Psychological Monographs*, 1931, 41, No. 184. Hilgard says in final conclusion to his study: "The limited experimentation on conditioning as a learning process and the theoretical objections which may be raised against considering the conditioned reaction to be the unit of habit, suggest that for the present it may be better to think of conditioning as a sample of learning rather than as the foundation of learning theory." p. 48.

in the face of alteration in stimulating conditions. This theory assumes that the basic units to which behavior can be reduced are simple, stereotyped reactions to simple, unequivocal conditions of stimulation and that neurologically, there lie back of these units of behavior certain very definite paths of conduction. Recent observation of human infants has begun to shake our faith in the simplicity and uniformity of what have always been regarded as the most stereotyped of reflex responses.<sup>7</sup> And recent experimental and clinical study of the functioning of the nervous system, particularly of the cerebral cortex, has brought into considerable doubt the theory of stable pathways.<sup>8</sup> While it is still true that behavior and mental processes reflect neural relationships, it now appears that such relationships are far less definite and stereotyped than was once believed.

Such critical points have no bearing upon the validity of the findings of Pavlov and of others who have utilized his methods. But they bear very directly upon the question as to whether the conditioned reflex experiment reveals the *real* neural mechanism back of all learning. Most of us would probably grant that the reduction of all cases of association to the conditioning of reflexes would be a pretty achievement. But apparently there is a genuine question as to the degree to which the simplest reflex remains intact upon conditioning and a further question as to how definite a mechanism this reflex was in the first place.

Perhaps the strongest assurance that a theory of association based entirely upon the conditioning of reflexes is untenable comes from the fact that staunch advocates of the theory have been inclined to use the concept of conditioning even where more is involved than the mere rearrangement

<sup>7</sup> M. and I. C. Sherman, *The Process of Human Behavior* (1929).

<sup>8</sup> K. S. Lashley, *Brain Mechanisms and Intelligence* (1929) and many papers by the same author.



of definite movements and secretings and definite conduction paths. Pavlov emphasizes the definiteness of reflex acts and recommends them as analytical units on account of that definiteness.<sup>9</sup> These claims are most vigorous, however, when he is talking generally and philosophically. Presumably it was observation that forced into his list of reflexes the *investigatory reflex* and the *freedom reflex*. While no one would be likely to deny that curiosity and a struggle for freedom can be conditioned, there would seem to be objections to calling them reflexes. Either these responses are very stereotyped, which we well know they are not, or else reflexes are not as definite units of behavior as Pavlov claims them to be. And, if the term *reflex* be extended to cover all behavior susceptible to conditioning, a new act of arbitrary definition has been mistaken for a theory of the make-up of behavior.

The argument might be advanced that Pavlov favors the reflex as the universal unit of analysis, not because it is a simple, highly stereotyped and definable activity, but rather because it is genetically primary. But even in infra-human forms the catalogue of innate behavior is exceedingly vague.

There are at the present time many psychologists who would emphasize the theoretical importance of Pavlov's conception without stressing the point that all learning consists of a rearrangement of reflex units. Accepting the common assumption that the law of contiguity is basic in all psychological connections some writers would name these connections through contiguity *conditioned responses*. Later I shall question the identification of all association with the condition of contiguity. But there need be no delay in asking why this ancient principle should have its name changed even by those Pavlovian admirers who would admit that all that is conditioned is not reflex. The answer is not far to seek. While the literal reflex theory of complex behavior is

<sup>9</sup> *Conditioned Reflexes*, Lecture 1, translated by Anrep, 1927.

hardly swallowed by contemporary behaviorism, Pavlov is regarded as less objectionably wrong than are those writers with whom the traditional theory of association has been identified. The fact that critics of the theory of association have a great number of writers any one of whom they may, with some justification, take as a bad example, has made the task of demolition seem relatively easy.

Although the objections to the theory of association have become thoroughly established in current psychological thought, there is perhaps no harm in listing them at this time and testing their relevance and importance.

It is said that association implies the existence of a force that binds together the associated items. There is no doubt that in times past psychologists have thought of some kind of pulling power lying behind the manifestations of association. Similarly, even to-day, many people read such a force into the gravitational principle. Yet, in the case of gravity, there is no need to change the name because the mind untrained in abstract pursuits feels a conceptual necessity for throwing out from earth to moon an invisible taut rope. And so in the case of this psychological principle of association there is no cause for dismay if the incautious mind thrusts a bit of figurative glue between the items that are functionally connected. If this propensity for making abstract relations concrete by placing *forces* into them is as chronic as the history of thought would indicate, there may well be uncertainty as to whether the use of the term *conditioned* will avoid the difficulty.

Of all objections to the theory of association, or perhaps better, to the term *association*, the most popular is the claim that its long use in connection with *purely mental* states makes it unfit for extension to phenomena of objective behavior. There seems to be a belief that association implies an awareness on the part of the individual in whom occurs

the association of the nature of the relatedness between the factors associated.<sup>10</sup> There is no disposition to deny that awareness of such relatedness may be a feature of psychological connections. There is simply a fear that the use of the term *association* will imply that such awareness is a fundamental and an invariable feature of such connections. Since the term *conditioning* was first used in an infra-human setting where claims of such awareness were not made (though, goodness knows, the awareness may have been present), it is thought that this term will tend to prevent the unwarranted postulation of awareness of relation. Now, any one who has ever taught introductory psychology to sophomores knows that there is a strong tendency among them to inject into any situation requiring psychological explanation a liberal measure of perceptual, affective, and ideational processes. And persistent effort is necessary to enforce allegiance to parsimonious methods. But why should one believe that a switch from association to conditioning will make these troubles less either with our students or with our imprudent colleagues? Other sciences find it wise to preserve lines of historic continuity. And there is a danger in making such a change. Not even psychology has such a shameful past that it will gain by disowning its legitimate ancestors. In fact, a science as beset by cults and fads as is psychology has double need for the intellectual poise that can be supplied alone by historical perspective.

Closely related to the argument that association has subjective implications likely to do violence to empirical facts is the point that association has so often been treated as a kind of connection between cognitive, as opposed to affective and motor, factors that one can hardly use the term without leaning toward an over-intellectual theory of mind and behavior. But: *Honi soit qui mal y pense*. Over-intellec-

<sup>10</sup> See especially Perrin and Klein, *Psychology* (1926), pp. 88-92.

tualism was a fault of British associationists. It is likely to be a fault of all future psychologists who devote themselves to the study of such phenomena as are largely intellectual. Still, the use of the term *association* is no more apt to force us into a one-sided conception of psychological organization than is the term *conditioning* with its own peculiar suggestiveness about the reflex composition of all behavior.

The treatment of association in its application to predominantly cognitive activities has also made possible the criticism that association has meant reification. Cognitive processes have always had a strong tendency to receive treatment as things rather than as events. Even among writers who would be quite willing to insist that cognitions are events and not things there is likely to be a certain carelessness of language that gives an apparent inversion to their real beliefs. But admitting the reality of this fallacy, it is doubtful whether this is a peculiar product of association psychology. Surely, events are just as likely to be called things whether the dominating category be sensation, or perception, or even reflex as they are where the principle of association holds the center of the stage. As a matter of fact, reification is not at all peculiar to psychology. The progress of human thought in almost every direction has called for a struggle against this tendency. The physical categories have almost all a history of fallacious reification. Yet the physicist is not inclined to alter his every statement of the laws of thermodynamics because heat was once regarded as a thing. And so we may become thoroughly alive to the fact that the perception of a candle flame and a movement of avoidance are not analogous to a pair of chunks of iron without feeling called upon to say that we have substituted a principle of conditioning for an outgrown principle of association. In respect to this practice of reification, the issue between association and conditioning has about as little to

do with the case as any issue could. The issue would be relevant if reflexes were things in a sense that other movements, imagings, feelings, perceivings are not. But a moderate sophistication in regard to current categories will give the reflex not a whit more thinghood than that possessed by other psychological processes or states.

In its usual form the application of the principle of association is analytical. That is to say, an effort is made to get into relative isolation as simple an associative connection as possible. This fact provides another basis for criticizing the conception of association. It has been the fashion of late to point out that most systems of analysis ultimately and in respect to their own subject-matter get into the predicament of all the king's horses and all the king's men—they cannot get Humpty-Dumpty together again. This claim can with a certain appropriateness be made of the analysis of matter into electrical charges, of the analysis of the organism into cells, of the analysis of consciousness into simple attributive values, and of all of the other recognized procedures. With equal validity one can show that analysis of a complicated group of psychological activities into simpler associated pairs, or groups, of activities will provide an insoluble problem if synthesis be attempted. One need not deny that an ideal analytical method would automatically provide the means of a complete resynthesis. But there are good reasons for questioning whether an analytical method falling short of such an ideal is to be set aside as a failure. If this perfection in the art of synthesis is not a *sine qua non* of an analytic procedure, then analysis into associative connections would seem justifiable. But if perfection in synthetic achievement is rigorously demanded, there must be rejected, along with the conception of associative connection, the electrical theory of matter, the doctrine of chemical elements, the cellular theory of organisms, and even the conditioned reflex.

It is, of course, possible to discuss the broad question as to whether analysis, as such, is not an inferior scientific procedure and one encumbered with grievous limitations. Something of this tone is to be detected in the writings of the members of the gestalt school. Although sympathy with any such viewpoint would throw the doctrine of association into a doubtful position, it does not supply grounds for substituting the conditioned reflex for the older and less formal conception. As a matter of fact, one can accept most of the limitations of analysis to which the anti-analysts call attention and still utilize the doctrine of association as a guide to analysis. But the theory of conditioned reflexes assumes that analysis is capable of exactly those achievements of which its philosophical enemies find it incapable. It is not the present purpose to discuss the status of analysis in science; a proper consideration of that topic would take us too far into the logic of science. The aim is simply to show that any criticism that may be applied to the doctrine of association because it is an analytical procedure applies even more appropriately to the conditioned reflex.

There is another general criticism of the conception of association that could not be accepted without casting into the discard all of the other conceptual instruments of scientific logic. The statement is sometimes made that the principle of association falls short of explaining *all* of those combinations of activities brought about in the course of individual experience. Not only are there cases of psychological activity for which the conception of association can provide no synthetic formula—there are also activities that resist anything further than the most superficial analysis into associative connections. It takes little thought to realize the irrelevance of this criticism. There is no major scientific theory, conception, or outlook that is actually universal in application. The universality attributable to physics by virtue of the

proposition that, if we knew *enough* physics, we could explain the important differences between Napoleon and Lincoln in physical terms, is one of the greatest fictions by means of which the human intellect has ever pulled the wool over its own eyes. All this simply indicates that the kind of universality lacked by the conception of association is a kind of universality not possessed by any of the basic scientific conceptions.

The outcome of the past few pages can be summarized very briefly. An examination of the prevalent criticism of the conception of psychological association shows that none of these criticisms is any more applicable to the theory of association than to most of the other widely accepted notions of science. If the methods of scientific thinking are fundamentally wrong, then the conception of association is also fundamentally wrong. But if the prevalent methods of science are merely marked by certain limitations, as are all other devices of human life, then the limitations of the theory of association need not be taken too tragically. And, if these criticisms of associationism do not give grounds for throwing the general conception out of psychology, they give even less grounds for substituting the theory of conditioned reflexes for the theory of association. Indeed most of the criticisms of association apply much more aptly to the theory of conditioned reflexes. The very definiteness that gives the conditioned reflex its appeal to so many minds makes it most susceptible to those criticisms which have been widely applied to the general theory of association, but which are relevant only to limited statements of it by this or that particular author.

If the conception of association is to be freed from the relatively futile types of criticism that have just now been under review, one accomplishment is necessary. There must be a clearer distinction between general and theoretical

outlook, on the one hand, and speculations and allegations as to fact, on the other. The thorough-going advocacy of the conditioned reflex as a substitute for association is based upon a belief that a certain fact, heretofore not fully recognized, is true—namely, that complex behavior is a composition of reflexes. But in what sense would this belief, even if substantiated, react negatively upon the traditional theory of association? The answer would depend upon what the traditional theory had said about the composition of behavior. Since some authors have treated association only in its application to ideas, it is possible to say that the traditional theory has claimed that all association is between and among ideas. That, however, is manifestly unfair. The authors who treated association only in its application to ideational processes did not do so out of any disbelief in the reality of other connections. In some cases conjectures were indulged in as to the fundamental neurological facts involved in all associative connections and in some authors these conjectures were a main interest. If Hartley and Thomas Brown can be placed within a single tradition there must be a central doctrine of association that rests upon the facts of organization through individual association which every sensible man will accept. Against the theory of association in its general, more stable form, then, the doctrine of conditioned reflexes raises no relevant objection. It merely claims that certain facts about the composition of behavior are true, which may or may not be true without embarrassment to the traditional theory of association. Upon first mention of those claims certain doubts were raised about their substantiation, but that was not strictly necessary. If we distinguish clearly between theoretical viewpoint and allegation as to fact, then the conditioned reflex conception may be considered merely as an attempt to give further speculative elaboration to the older and more general theory.



## Association and Adaptation

In the case of the conditioned reflex hypothesis the attempt has been made to find a basic conception for psychological theory which would be more definite and specific than the principle of association. There also have been efforts to found psychological theory upon a conception of adaptation which is assumed to be more general than that of association. This has been true of the so-called functionalists. In fact, the functional school has come to be known more for the centrality in its thought of the conception of adaptation than for its other outstanding characteristic—its advocacy of a dynamic, as opposed to a cross-section, viewpoint.

In writers of the functional school there has been a wide acceptance of the concept of adaptation as a fundamental principle. Association has been treated as a manner in which the organism's struggles for adaptation are attained. The relative positions of the concepts of adaptation and association have rarely, if ever, been treated explicitly by the functionalists, but certainly the use to which these conceptions are put would bear out this claim regarding their actual position in the system. It should also be said that the functionalist writers have not always called the principle of association, as defined in an early part of this paper, by that name. Such expressions as *habit formation* and *learning* have frequently been used to help cover the ground. However, the intent to recognize that broad principle, which we have on historical grounds called *association* is clear, and clear also is the subordination of this principle to that of adaptation.

There are other psychologies than the American functionalism in which a conception of adaptation is central. This is true of the purposive theory of McDougall and of the

several varieties of psychoanalytic writers. But in these cases association is no longer looked upon as the fundamental manner in which adaptation is achieved. Writers of these schools hardly deny that there is such a principle as association, but they would certainly give it more restricted application than do the functionalists.

In so far as theoretical psychology is to be set up with a biological outlook and in so far as biological science is to be organized around a theory of evolutionary adaptation, one can see a certain reason for giving a central place in psychology to the conception of adaptation. True to their pragmatic background, the functionalists have conceived of conscious life as a phase of one larger biological life the central business of which has been that of getting on in the world.<sup>11</sup> From this perspective one can discern not only why the concept of adaptation should be fundamental, but also why it should be more fundamental than the conception of association, toward which the functionalists have been more than hospitable. In biology at large there are other ways of achieving adaptations than through the formation of new psychological associations. Through the process of natural selection better conditions of adaptation can presumably be achieved. Through the process of regeneration adaptation of form may be gained within the life of the individual organism. The formation of associations is thus only one of several manners in which biological adaptations are reached.

Even if one does not choose to admit the importance of biological considerations in determining the nature of psychological theory, it is possible to find some justification for giving the notion of adaptation a more fundamental position than that of association. The claim can be made that new or improved states of adaptation often arise without

<sup>11</sup> This "getting-on" includes the achievement of intellectual and aesthetic ends as well as of those usually thought of as practical.

giving any evidence that they are the results of the formation of associations. It seems to be just such a claim that the members of the gestalt school make regarding the phenomenon of insight. They stress the fact that, in this case, the behavior is both novel and appropriate and yet not the result of association formation. And it seems to be on similar grounds that Spearman lays down such fundamental principles as the education of relations and the education of correlates.<sup>12</sup> Here we have categories for acts the novelty of which might exclude them from an explanation in terms of association and yet acts which may be highly adaptive—may indeed be the achievement of a condition of adaptation. I do not wish to insist that adaptation is a basic conception for either the gestalt school or for Spearman. But both do imply that adaptation can be reached through other mechanisms than that of association, and, if this be true, it is certainly an argument for giving the conception of adaptation a more fundamental position than that of association.

There are, however, arguments against the fundamentality of adaptation as well as for it. If every scientific point of view is sooner or later to be brought to bear in the solution of life's practical problem, the postulation that psychological (and biological) occurrences tend toward an improvement of adaptation may prove the worst road to service. The great problem of practical psychology is that of discovering why human behavior sometimes works toward better adaptation and at times in the reverse direction. Now, if one assume that the most usual characteristic of human activity is its achievement of a better condition of adaptation, one is forced to explain adaptive behavior by the barren statement that it is typical. Under these same circumstances one might, of course, explain maladaptive behavior simply by saying

<sup>12</sup> *The Nature of Intelligence and the Principles of Cognition* (1927), Chapters V-VII.

that it is atypical. It is a more usual procedure to seek out some sense in which the maladaptive behavior is not maladaptive, but adaptive. This is the primary trick of the psychoanalysts. Finding a person behaving in a manner that balks his own desires, they search for some other desire that may be fulfilled by the behavior. Since performing any act is, in a way, doing what one wants to do, there is little difficulty in showing that the maladaptive act was, from one point of view, adaptive. And, after this has been done, a fine thrill of scientific accomplishment may be enjoyed. But what has been accomplished in the way of explanation is exactly nothing at all.

If light is to be cast upon adaptation and maladaptation it can be done only by means of an analysis based upon a conception largely independent of that of adaptation. If one works in terms of the conception of association it is possible to show how adaptive and maladaptive activities have developed without one's being forced to the ridiculous assertion that both are, after all, adaptive. If adaptiveness, or the reverse, is what psychology would explain about human activity, then adaptation is the chief problem with which psychology is concerned. But to postulate one's central problem as one's basic principle of analysis is to end the game before it is begun. If variation of adaptability sets the problem, explanation can come only in terms of some principle, like that of association, which contains no more bias for behavior that is adaptive than for behavior that is maladaptive.

The postulation of adaptiveness as the basic quality of behavior defeats the purpose, then, of a practical psychology. But what about the adequacy of such a postulation for a psychology that forswears practical service? Although logical enough, this question is almost purely academic, because it is largely in the writings of psychologists who admit at

least a remote interest in practical applications that the notion of adaptation has had any important place.

Another argument against founding psychological theory upon the conception of adaptation involves the necessary ambiguity of the notion. In order to recognize adaptation when we see it, we must be cognizant of a long and complicated list of human valuations. Within this list we shall find contradictions. In other words, if we engage in the business of predicting behavior in terms of the adaptiveness toward which it tends, we may find it necessary to predict that it will run in opposite directions simultaneously.

With these points in mind, it would seem wiser to base our analysis upon some concept like association which is neutral in respect to the values in the achievement of which science is presumably an instrument. It would also seem wiser to postulate a principle of less ambiguity than that of a tendency of behavior toward adaptation. The notion that human acts get functionally connected and disconnected in the course of the individual's experience is certainly clearer than is the notion of a tendency toward adaptation.

Although the status of the concept of association is systematic psychology cannot be made out fully until its relations to other fundamental notions, such as that of adaptation, are determined, it is quite possible to discuss association without admitting either that it should or should not be postulated as the fundamental principle of psychodynamics. In order to follow, and perhaps to agree with, the remainder of the present essay, it is not necessary that the reader should have agreed that the conception of association offers a firmer foundation for psychological theory than does that of adaptation. It is merely necessary that he acknowledge association to be a legitimate scientific conception of very general applicability.

## The Analytic and the Synthetic Views

It has already been pointed out that association is usually conceived of analytically. The idea of white is said to be associated with the idea of snow, with the hearing of a bell with the act of salivation. Critics of associationism have made a great deal of the fact that such statements are inevitably inaccurate, because they give the impression that the members of these associated pairs are the sole determiners of the closeness of their association. These items are but artificially abstracted parts of essentially unitary situations.

An interesting alternative to the analytic view is offered by Hollingworth.<sup>13</sup> Instead of beginning with two relatively simple activities and describing the conditions of their association and dissociation, he takes his start with a large situation and lays down the conditions of the maintenance and dissolution of that large situation. He treats the phobic reaction to a sudden but harmless sound as the partial reinstatement of a complicated psychological situation—that of actual battle. The more conventional procedure would, of course, consider the association that had previously been formed between a loud sound and the reaction of terror. Now, obviously, the original association involved an elaborate complex of acts and experiences, and the sound-terror association is sure to receive a very inadequate explanation if its originally complicated setting is not taken into account. But there is some question whether the attempt to arrive at pairs of associated items has not a certain economy as well as a certain danger. Complete situations can be starting points for scientific study, but by the very nature of the case they can never be more than that. If clarity is to be

<sup>13</sup> *Psychology* (1928), especially pp. 35-45.

secured, completeness must be sacrificed. The integrity possessed by complete situations is beyond human descriptive powers. Therefore it is a questionable model for scientific description. One might recommend that physics deal with real sticks and straws as they actually fall rather than with those abstracted conditions that are described by the law of gravitation. Yet physics is not asked to photograph reality in this way, because it is believed that, so long as physics recognizes that an abstraction has been made and what the nature of that abstraction is, the law of gravity will gain to the degree of its detachment from complete reality. The hearing of a bell and the act of salivation, the idea of white and the idea of snow are more or less arbitrary units of activity, it is true, but, largely because they have been separated from the whole of experience, we have been able to handle them.

The analytic view of association—the view that attempts to reduce psychological phenomena to as many simple pairs, or sets, of items as possible—need not ignore the influence of setting upon the relatively simple associations. Indeed, careful writers have not, as a rule, ignored the fact that the precise nature and strength of any given association depends upon its context.

The objection to the conventionally analytic view of association is part of a more general philosophical revolt against analytic methods in general. The opinion has already been expressed that if the current objections hold for one of the accepted methods of scientific analysis, they also hold for the others. However, these objections will soon have lost their appeal. The doctrine of emergence, the theory of gestalt, vitalism—all these and their raucous cousins—are concerned with the fact that reality is more than a simple combination of the broken bits that come out of physical, biological, or psychological analysis. Of course it is. If it

were not, we should have, instead of sciences, an abiding ontology. There is a real question, however, as to whether the sciences should throw over their methods of analysis, which are, after all, their main achievements, in favor of some synthetic view which will guard them against artificial abstraction and keep them close to living experience. Indeed, there is some reason to believe that the full, concrete, living experience which some would have science seek is only a poet's rainbow-end. The failures of science lie in the facts that her abstractions are not always well-defined and that inconsistencies arise in her discourse, but these are hardly to be cured by recourse to that mystical, unanalyzed real reality about which, by the very nature of the case, nothing definite can possibly be said.

To return to the concept of association, it is fair to conclude that although the analytical character of the conception has been criticized and although certain attempts have been made to save the essentials of associationism by stating the doctrine with reference to whole, concrete situations, nevertheless the notion has usually been applied analytically and its future development probably lies in that same direction. Exactly what is meant by a thoroughgoing psychological analysis in terms of association will be somewhat developed in the pages to come.



## Chapter II

### WHAT ARE ASSOCIATED?

#### Limits of Psychological Association

THE ideal of associational analysis is to segregate, from any complicated psychological event, as many associated items as possible and to state as fully as possible the conditions accounting for their association. Or, to view the same matter from a slightly different angle, the ideal is to formulate as accurately as possible the principles applicable to associations among and between such items. In order that this procedure can be clear the first requisite is to state the nature of the items, or acts, that become associated.

It should be evident, in the first place, that the units toward which psychological analysis is directed must be units of a restricted type. Nature and experience are full of connections that are not psychological. The cold and December, the fire and the smoke, the prison and the criminal, the lady and the lifting of one's hat—all represent associations of a kind, but ordinarily they are not treated as psychological associations. Someone's sense perception of cold and his idea of December are stuff whereof psychological associations are made; but the workaday cold and the workaday December are not. It is possible to claim that all of the organizations and connections about which anything is known rest finally upon the psychological states or activities of individuals, but many natural groupings can be treated more importantly than by showing their dependence upon the principles of

psychological association. There is a psychology of internal combustion engines, but it would furnish an awkward and inaccurate guide for the practical control of such machines. That such a limitation is not a peculiar weakness of psychology is easily shown. The entire realm of the physiological is presumably encompassed by the electrical theory of matter, yet to most physiological detail this theory offers no practical approach.

There are, of course, many legitimate meanings of the common word, *association*, that do not pertain to the same word when it is used in its technical, psychological sense. Since all of the sciences use words in special, restricted ways, such confusion as arises in the case of the term *association* is not to be blamed uniquely on the term itself. Still, we may as well face the fact that confusion is there. We shall not soon be entirely free from scientific writers who attempt to discover all of those meanings that a common word may have and who then assume that those meanings are necessarily among the term's technical contents. Such writers are constantly forgetting the necessary arbitrariness of scientific terminology. They seem to regard Nature as an infallible superdictionary.

As long as psychology operated in the simple universe of nineteenth century science, there was little difficulty in distinguishing between psychological connections and connections of other sorts. The individual was conceived to be immersed in the world of the professor of physics. This world, being the real one, was *pre-psychological*, wherefore there was no temptation to call the laws of its organization *psychological*. That obstreperous race, called *philosophers*, did so now and then, but psychologists had too strong a father-hatred to take advantage of this ally. The psychologists confined their operations largely to mental states which neither they nor the professor of physics thought were very

real. So, since there was little call for the physicist to deal with mental states and since the psychologist was humble and reticent, the basis for confusion was slight.

In these present days of criticism, the situation is not so happy. Physics is only a one-sided picture of the world in which the psychological individual operates. That world is also social and even psychological. Thus, we find a writer like Kantor telling us that associations may be formed between stimuli and stimuli.<sup>1</sup> If he had said between stimulations and stimulations, thereby meaning phases of individual activity, one could understand this application of psychological principles. What we have in Kantor's case and in that of Hollingworth, when the latter speaks of associations between "primal events in nature," is a kind of inversion of the stimulus error, but the "error" in these recent writers is willfully committed. They are frank enemies of instrumentalistic and conceptualistic views of science. They are attempting to describe Nature in the nude. Facts ordinarily treated at different levels of discourse become forced into the homogenous world of concrete reality. Consider Kantor's statement that . . . "perceptual activities are those in which the person reacts to explicit stimuli."<sup>2</sup> There would be little difficulty in showing that the term *stimulus* has come, by very general consent among psychologists, to refer to an object or event, which though it is related in discernible ways to psychological phenomena, is in itself to be described mainly in non-psychological terms. Such a stimulus might be a factor in perception but certainly it could never be, as stimulus, explicit in perception. In fact, one of the chief problems of perception has been created by the lack of explicitness of the stimuli involved in such activities. Similarly, for the operation of the principles of psychological association.

<sup>1</sup> *The Principles of Psychology*, I, 47-56 and 321-324.

<sup>2</sup> *Ibid.*, I, 250.

Stimuli have traditionally been considered as objects and events in so far as they are free from explanation in terms of such principles. Now, it may be defensible to discard conventional usages in favor of those more in accord with one's favorite philosophy. But the philosophy created by the forward movement of science, itself, is more likely to prevail than that which a few scientists devise in order to justify their own philosophic theory. Thus, it is questionable whether psychological association is likely to be extended to cover all "primal events in nature" or such facts as have customarily been treated within the special category of "the stimulus." But before attempts at this extension cease there are likely to be confusions aplenty.

A clear delimitation of the application of the principles of association will depend upon a distinction between what might be called the internal and the external categories of psychology. The internal categories embrace sets of facts to the degree to which the main relationships among those facts are psychological. Perception is such a category, and so are feeling and remembering. This proposition would be generally accepted. Other categories, such as that of bodily movement, are of more doubtful status. The external categories are based mainly upon the application of borrowed principles. The physical world in which all individuals do their acting is a case in point. This world is assumed to be organized along the lines of physical principles. Certain of the external categories have been developed by the psychologist for his own purposes. The worlds of mazes and of nonsense syllables are only unimportantly physical. The principles of their organizations are largely inventions of the psychologist. Yet even such categories are external in that, after investigation is under way, they are taken as given and not translated into the central terms of psychology. Some of the external categories are very dubious affairs. For instance,

psychological activities may be brought into relationship with the classifications and distinctions of everyday life. Nevertheless, the marks of externality of a category are, themselves, clear enough to be recognized.

In the interests of precision of thought the application of the conception of psychological association should be restricted, then, to those most distinctly psychological situations where this particular conception is of first rate consequence. There are important things to be said about the world of physical stimuli—things important for the psychologist, himself—without our adopting the practice of Kantor in which he classifies stimuli in terms of the strictly psychological situations into which they enter. When one talks about associations among stimuli one is fairly sure to neglect to say anything about those nonpsychological relationships among these stimuli which should be the base line upon which are plotted the psychological phenomena to which the stimuli are related.

### Associations as Dispositions

The answer to the question, "What are associated?" can hardly be reached without raising another question of a definitely prepsychological sort. I refer to the time-honored issue as to the status of associated items when they are not under observation. It has long been the custom to speak of associations as though they were connections between objects and then promptly to add that these objects are really not objects at all, but events. The distinction between objects and events, however, is mainly that the former have a relatively stable endurance while the latter go out of existence when they go out of observation. Thus a very embarrassing situation is created. Associational relations are functional relations of at least a degree of permanence, yet how can

such relations possibly exist between items of almost no permanence whatsoever?

There are three major methods of escaping this dilemma. First, one may state that the associated items are objects after all. Between appearances they reside in the obscurity of the unconscious. In its literal form this doctrine is certainly not widely subscribed to by scientific people. It is kept alive mainly because it readily captures the popular mind.

Second, one may say that all of the events through which the principles of association are manifested are activities of the brain or nervous system. The brain is an object and has object-like, or stable, characteristics. What is more likely, then, than the theory that the seeming permanence of associative patterns is based upon the establishment in the brain of relatively permanent characteristics. Thus, we have the *neurogram* as the reality lying back of psychological associations. This theory has a wide appeal and has been accepted by a good many modern psychologists. An important objection to the theory is that it endows the brain with all of the characteristics necessary to explain the observed facts of association and that it throws little light back upon those facts by assuming that their preservation is neurological. A profitable theory leads to the discernment of relations not discovered before the theory was formulated. Such results have been very meager in this instance. However, this line of argument falls far short of demolishing the theory and there is every reason to believe that men will continue to visualize the permanent relations between their activities as embodied in relatively stable characteristics of the brain. It is far more than mere conjecture that psychological retention is intimately related to the condition of the brain. There is difficulty, though, in the naïve assumption that psychological activities are any more neural when in a state of potentiality than when in a state of actual occurrence.

A third possibility lies in the doctrine of psychical dispositions. The vase upon the mantel has as one of its characteristics the fall it would take if touched by the lightest breeze. The breeze may not come for months, yet the capacity to fall five feet and break itself to bits remains. But where does it remain? We may look at the vase, back of it, and under it without discovering the potential fall or any little cupboard in which that fall is stored. In such a case as this it seems ridiculous to force reification upon such an event as a fall simply because the possibility of that event's occurring has a relatively prolonged endurance. Some might claim that the spatial position of the vase contains within it the only lasting reality that there is in the falling and breaking. Such a statement would be analogous to the proposition that the gap from certain of Lincoln's past experiences to the composition of the Gettysburg address was filled only by the endurance of the arrangement of certain objects (possible neurones, possible electrons) in his brain. It is fair to say that object-worship of this type is at present in great disfavor in physical science. The theory that every temporal series of events is cemented together by objects is not regarded as seriously as it once was. Where science discerns a number of events, interrelated in some way, but separated in time, the first task is to give a clear empirical statement of the occurrences. The next step is to extrapolate and to interpolate through inference and through investigation in order to extend the series and to give it as great continuity as possible. But there is now less certainty that, in the progress of such search, object-like facts should be valued above those which are called events. The application of such a frame of mind to the problem of the permanence of associations is not difficult to see. The known facts are the groups of acts or states that occur and recur. What endures is a likelihood of certain acts appearing in certain orders and

contexts. The Frenchman's actual possession is a capacity to say *Bon jour!* when he sees his friend. And there should be no objection to talking about such capacities or dispositions as if they were real. They are as real as the potential energy of the vase upon the mantel. They are even as real as the stubborn presence of a carved inscription on a block of granite.

Acceptance of the conception that psychological dispositions have as substantial a status as any other variety of potentiality should put a permanent end to the cry that the theory of association assumes such events as images to be permanent objects. Many writers, it is true, have spoken as though psychological activities endure, as such, from one occurrence to another. But such statements have not fooled anyone regarding the actually observable facts in the situation. We do not worry for fear the innocent mind will assume that his walking, as walking, will be always with him because he is said to have acquired that act. And why should we worry about his interpretation of what is retained after the mastery of a list of nonsense syllables? What he will really mean will be capacity rather than act, even if he does not trouble to say so.

The bearing of the theory of psychological dispositions upon neurological dispositions has not been treated very wisely. Certain writers find in this doctrine a defense for writing a brainless psychology. There are a number of possible explanations for this attitude. Perhaps the chief one at present is related to a revolt against the belief, recently current, that psychological events are epiphenomena and neural events their real substratum. In order to prove that this is not the case, psychology may be written with painstaking avoidance of any but the most general reference to the nervous system. Another motive for omitting neural considerations is to be found in a strong desire for homogeneity of



description. Unwilling to accept any of the theories of the psycho-physiological relationship, many writers keep away from facts which force that relationship into prominence. In my mind all this studious avoidance of neurology is merely a gallant philosophical gesture. In the midst of it all there come forth great studies like those of von Monakow, of Head, and of Lashley, which are certainly not to be argued out of importance. Although it may be an arduous undertaking, clarity as to psychological conceptions must be won by other means than by the adoption of glasses opaque to neural rays. Science likes unity and consistency, but her history teaches us emphatically that her first love is fact.

There is no earthly reason why we should not recognize the genuineness of associations among psychological dispositions and, at the same time, keep our minds open regarding neural theories of the associated complexes. We can allow the entrance of neurological facts and hypotheses into the discussion of association without assuming that behind all associations among acts there lie the real inscription-like modifications of the brain. It may well be that the neural facts will turn out to be much more event-like than object-like—that the neurological interpretation of psychological retention will have much more to say about neural events and neural capacities for activity than it will about neurograms. All of the recent evidence for the flexibility of functioning of the cerebral cortex and such general biological theories as Child's gradient conception of individuality indicate that the neurologist, himself, is little likely to think of the interrupted series of psychological events as having its permanence and continuity in a fixed and lasting imprint upon the tablet of the brain. If a bit of thinking reveals that there is more than neurology in the psychological universe, a very little more will enable one honestly and interestedly to face such neurology as is obviously there.

I am not arguing for a theory of disembodied functions or for a theory that gives dynamic relations an ultimate supremacy over structures and objects. The point is simply that the reverse of this doctrine has been dominant, whereas the situation calls for a complete cessation in science of the attempt to found reality upon things in preference to events or upon events in preference to things.

### General Terms for Associated Items

While we may admit neurological matters into the conception of association, the most bountiful and important facts involved are still facts about psychological activities and their functional interrelations. Since this is true, the next problem may well have to do with an investigation of the content of the expression, *psychological activities*.

In so far as they are associated, psychological activities are of two sorts. To take over a terminology suggested by Hollingworth, there are instigating activities and instigated activities. Whenever we speak of association we assume that a certain act, or group of acts, has become able to arouse another act or another group of acts. This distinction is at the very heart of the notion of psychological association. The complexities of psychological life are such that some acts may well be instigated in their relation to certain acts and instigative in their relation to others. But the distinction does not have the purpose of a permanent catalogue. It merely points out a usefully abstract mechanism.

Although there is no little evidence that the English associationists saw wider applications of their principles, the statement of them usually was made in terms of ideas. As long as this was the case, small effort was made to distinguish between instigating and instigated items. Of course, the concept of association has always contained the notion

of instigation, but as a rule the fundamental terminology has had no provision for keeping that notion explicit. But with the development in this country of the stimulus-response point of view, there has been a tendency to correlate the distinction in question with the distinction between stimulus and response. This correlation is usually stated without argument and has thus far escaped any extensive criticism.

Carr's statement of association in terms of stimulus and response is clear and typical, and may well be taken as text in evaluating this general practice.

"Any two items of experience are said to be associated whenever they are organized in a stimulus and response relation, *i.e.*, when the presence of one will stimulate or arouse the other. Any adaptive activity may be regarded as an associated sequence of analytical elements, for any part of this act is a stimulus to that which follows, and conversely any part is a response to that which preceded."<sup>3</sup>

Now the acceptance or rejection of such a statement of the conception of association must be based upon whether or not the notion of stimulus and response can be generally identified with the instigating and instigated items respectively in associative organization. Carr says that the identification can be made, but he is careful to add that he means by stimulus any earlier part of a total organized activity and by response any later part of such activity. In other words, the terms *stimulus* and *response* are defined as the more general terms *instigating* and *instigated* have been defined. This makes the whole issue seem to be one that should be settled purely on the grounds of convenience. But actually, it is much more than that. The stimulus-response theory has many ramifications that need to be taken into account. And the terms themselves have customary usages that can hardly be neglected without inviting confusion.

<sup>3</sup> *Psychology* (1925), p. 102.

In another connection (pp. 29-31) I have dealt with the conventional meaning of the stimulus in psychology. Ordinarily the stimulus has been regarded as an affair of extra-psychological character. The radiant energy that excites the retina is a stimulus by almost universal consent. An object, such as a cylinder that one lifts, when endowed with its common-life properties, is also considered as a stimulus by many psychologists. But the extension of the domain of the stimulus, until every fact relevant to psychology is encompassed, has come as a result of the relatively careless application of a broad viewpoint rather than through careful decision. Although the final judge in this matter must be history, and although the less precise conception of the stimulus may prevail through sheer weight of numbers, I cannot believe that psychology will become clarified by such a result. It is only by neglecting what the stimulus has ordinarily included and implied that the instigative item in an associative connection can in general be called a stimulus. I should even go so far as to say that one ought never to speak of a stimulus as being an item in an association. A *stimulation*, which is that phase of a total activity in closest relationship with the physical, physiological and other extra-psychological affairs, may be spoken of as associated, because it is an activity of the individual organism, but there is a world of difference between the stimulus and the process of stimulation.

The conception of response has never been as definite as that of the stimulus and for that reason the objections to including in it the very general conception of any associatively instigated item are not so serious. The type of response that is ordinarily considered most typical is the overt movement of skeletal musculature. However, we find writers of a dynamical frame of mind insisting that even a "sensation is a

response.”<sup>4</sup> Thus, the term *response* may be merely a synonym for activity. As such it might be harmless enough as a name for any subordinate term in an associative connection, if it were not for the prevalence of certain motor theories of consciousness and of thought. Because of the fact that the overt muscular movement is so often regarded as the typical form of response and other forms as mutilated or curtailed responses, the too general use of the term *response* may imply the acceptance of one or another of these motor theories. I am not arguing against these theories as theories. Indeed, I believe that there is considerable evidence in favor of the motor theories. I am simply insisting that they still are hypotheses, whereas the fundamental outlines of the associative process can be stated prior to and independently of the introduction of merely speculative considerations.<sup>5</sup>

If, then, there is a need for a general terminology which takes account of the fact that in any associative connection at least one item is functionally subordinate to at least one other item, I believe that we shall do well to avoid the implication that this distinction is what is ordinarily meant by the distinction between stimulus and response. There will be far less danger and quite as great accuracy in the historically very neutral expressions *instigating* and *instigated* items or acts.

### Associative Instigators

Having considered the most general distinction between associated acts, we may now proceed to further and more

<sup>4</sup> R. S. Woodworth, *Psychology* (1921), p. 187.

<sup>5</sup> A recent and important exposition of a motor theory is that of Washburn, *Movement and Mental Imagery* (1916).

Another excellent statement is that of Langfeld, “A Response Interpretation of Consciousness,” *Psychological Review*, 1931, 38, 87-108.

detailed distinctions. First, the question may be raised as to what specific kinds of activity can serve an instigative function. After that, an attack may be made upon the closely related problem as to what specific kinds of activity can be aroused through associative instigation. These classificatory investigations will touch a number of disputed points that must be squarely faced if the conception of association is to be modernized and clarified.

To begin with, the stimulus may be dismissed as an associative instigator. As previously stated, the stimulus is an important initiator of psychological activity, but is not, in itself, such an activity. Since the stimulus is something marked by characteristics independent of any individual, it cannot be said to become involved directly as an item among an individual's acts, or states, or conditions. One can readily see why this point is frequently overlooked. The stimulus is often a known factor in an experimental situation, while the initial stages of the activity aroused by it may be quite obscure. When, therefore, an association becomes established between the early phase of some sensory process and some movement or cognition, that early phase of activity tends to become identified with the stimulus. Thus, it would be natural and easy to think of an association between a relatively complex set of air vibrations coming from the right and a turn of the head in that direction. But, of course, all would admit that there lie between the associative connection and the stimulus those other types of relationship involved in the structure and responsiveness of the anatomical sensory mechanism. To speak of the association between a stimulus and a movement fails to bear down as closely as possible upon such facts as are actually known to be involved. This is probably a more serious and fundamental objection to the inclusion of stimuli among associated terms than the objection based upon the somewhat philosophical

distinction between the internal and external categories of psychology.

The activity, properly included among associative instigators, which has the most direct contact with the stimulus is the sensory process. This statement raises certain fundamental issues. In the first place, we may ask whether we ought not to speak of perceptual, rather than of sensory, process. Are there, in other words, actually precognitive, or subcognitive, processes that act as associative instigators? Related to this first question is another which asks whether, by sensory process, is meant the sensation of introspective psychology.

The analysis of perceptual activity is, itself, the best method of revealing the existence of occurrences at a precognitive or even at an unconscious level, which associatively instigate other psychological activities. The sensory processes instigating the experiencing of a visual third dimension are only very inadequately to be observed in the actual act of cognition. It is hardly necessary to cite the multitude of other examples of actual, sensory processes lying beneath the superficial facts apparent in any given act of perception. When sensory process and perception are distinguished there is a tendency to describe them as temporally disparate, the sensory process being an actual forerunner of the perception. This description is accurate enough from the standpoint of conception. We need only avoid falling into the assumption that the temporal disparity is observable.

The answer to the first question really furnishes an answer to the second. The sensory processes cited are obviously not sensations, as that term is used by careful writers. Although the sensation has come to mean an abstracted aggregation of attributes rather than any concrete fact, it remains an abstraction of that which is actually a part of

awareness in a way in which "unconscious" background activities are not. Many of the sensory processes that are to be detected in the visual perception of depth are not sensations but merely sensory processes.

It is quite a different question as to whether sensations can *ever* be thought of as associative instigators. My answer to this would be that they certainly can be. No amount of introspection would reveal the timbre difference instigative of the auditory perception of direction. Yet a timbre difference can be proved to be present. But the case is not always of this sort. An instigative sensory process may be discovered through introspection. I may identify a book largely because of its gray cover and then discover the process "gray" by introspection. The process discovered in this situation is not, however, one that before its discovery was unconscious, as the timbre difference was unconscious. It was precognitive rather than unconscious.<sup>6</sup>

The excuse for subsuming the sensation, a dominating feature of radically introspective thought, under the more general conception of sensory process is that the more general conception is dynamically more important. And the main purpose of this study is to achieve some clarity in regard to the dynamic principle of associative connection. My reason for not neglecting a conception such as that of sensation is that I believe in its usefulness. I believe that it aids us in holding in mind certain important distinctions among psychological activities as well as certain dimensions of observable experience. The reason for not relegating all sensory

<sup>6</sup> It is so fashionable nowadays to reject sensation altogether that, perhaps, one ought to apologize for not joining the crowd. My own belief is that it is one of the clearest and most defensible conceptions ever formulated in psychology. The criticism of it on the grounds that it is never a fact, but always an abstraction, has been based upon a very naïve view of what scientific facts are. See L. Carmichael, "An Evaluation of Current Sensationism," *Psychological Review*, 1925, 32, 192-215.



processes to the level of physiological description is the simple one that we can identify a great many of such processes in respect to their psychological functions—such as reactivity to binocular disparity—but about few of them can any specific physiological remarks be made.

Although an instigative function may be attached to pre-perceptual, sensory processes, complete perceptual acts may also play the rôle of instigator. This is commonly recognized to be the case in the initiation of thought activities and in the arousal and control of bodily movements. Associative instigation on the part of a perceptual process may also determine the direction of further activity at the perceptual level. The conventional description of association has shown one idea instigating a successor. No essential revision of this description is necessary. In manifesting the principle of association one ideational process may instigate another ideational process, and it may also instigate perceptual activities and even bodily movements. Although it is highly desirable to entertain hypotheses looking toward the explanation of ideational processes in terms of movements, postures, brain states, and so on, one can keep clear as to the fundamentals of association without taking sides in these speculative controversies.

Lately there has been increasing recognition of the fact that affective activities enter into associative organizations. The phenomena of mood are perhaps best illustrative of the importance of affective states as associative instigators.

Associationism is sometimes criticized upon the grounds that total situations are forgotten and that the conception is stated as though it applied only to connections between isolated items of activity. The inclusion of affective states among those having instigative functions is in direct appreciation of the fact that all of the detailed connections of behavior and experience occur in a context. The actual condi-

tion of affairs is given still more adequate treatment when the determining tendencies in general are admitted as associated and associable items. Of course, as long as these determining tendencies are regarded merely as factors modifying the operation of the principle of association, they may be looked upon as marking off definite limitations for the principle. But what needs stressing is the fact that the functioning of such tendencies as a kind of background for more readily isolable items is, itself, subject to interpretation in terms of association. The fact that one is in a state of depression when he scratches his finger has a great deal to do with the prompt thought of blood poisoning. Another mood might have brought the thought that the coagulation of blood is a blessing. But this is not an illustration of the limits of the associative principle, as some would have us believe. The important point is that the principle of association is in this case as clearly manifested by the instigative function of a very general condition as it is by the instigative function of a highly specific activity, the experiencing of the scratch. There is nothing in the essential conception of association that precludes the instigative functioning of general as well as of specific acts or conditions.

Among the types of activity nowadays incorporated into psychological description, there are some that probably do *not* function as associative instigators. Among these are muscular movements and postures. There is a certain temptation to talk about concatenations of movements as though the associative principle were applicable to direct connections between movement and movement. But our best judgment would seem to indicate that intervening sensory processes (even if only subliminal ones) are required for such connections. The initiation of a sensory process—in so far as that process is actually sensory and precognitive—is a matter of the effect of the stimulus upon sense mechanism and does

not require for its explanation the principle of association. The instigation of movements in such a series or complex may indeed be an associative instigation, but, if so, the instigative function is probably performed by a sensory process and not by another movement.

A similar criticism is also applicable to the notion that glandular reactions have such a function. Although glandular secretions may alter the conditions of the nervous system without the intervention of the sensory type of reception on the part of the nervous system, it is the effected alteration rather than the secreting activity that enters into the rôle of associative instigator.

The reader will, perhaps, have seen that the present argument is related to the earlier one regarding the status of stimuli in associative connections. The movements and secretions of an individual get back into the psychological realm only as a result of the arousal by them of a sensory process or nervous activity, and the principles determining this arousal are not contained within the conception of psychological association. If, then, one talks about the instigative functions of muscular movements or glandular secretions, one is deliberately ignoring certain problems of organic receptivity. There are, of course, practical situations, or at least situations in which the fundamental principles of psychology are of secondary consideration, and these will stand a short-circuited description of an associated complex. A too great affection for the convenience of such short-circuits, however, is likely to lead a writer to banish the intervening facts as nonpsychological.

The objection here raised against conceiving of muscular movements and glandular secretions as associative instigators might meet the reply that it has a largely physiological basis and is, therefore, not of primary importance to fundamental psychological theory. A full consideration of this

point would constitute too long a digression. Perhaps it is enough if I again state the belief that the attempt to free even the basic tenets of psychological theory from the obtrusive fact that there is a physiological organism involved has never been anything better than a make-believe. Psychology has listened to frequent admonitions to keep her fundamental conceptions free of physiology, but obedience has never been at all general.

### The Associatively Instigatable Processes

We may now turn to those psychological activities which are susceptible to associative arousal or instigation. Ideas, feelings, general sets, muscular movements and glandular secretings can certainly be initiated in this way. Perceptions are dependent upon the instigative capacity of sensory processes.

But sensory processes are *not* to be conceived of as associatively instigated. It is important that we have some terminology by means of which we can deal with that abstracted phase of psychological activity which is directly dependent upon the conditions of stimulation and of sensory mechanisms. To speak of sensory processes as associatively instigated is to confuse the conception of sensory processes with those of more elaborated operations and thus largely to destroy the usefulness of the former. Another point at issue is well illustrated by Hunter's paper on association published in 1917.<sup>7</sup> That author, who was then only a very moderate behaviorist, undertook the timely task of showing that, whereas the fundamental principle of association is one of the permanent achievements of psychology, there is need for acknowledging that this principle applies considerably

<sup>7</sup> "A Reformulation of the Law of Association," *Psychological Review*, 1917, 24, 188-196.

beyond those cases in which the instigated activity is ideational. He called attention to such associated series as occur in maze running. But he stopped just short of making the needed extension of the principle of association. He stressed the fact that the associatively instigated movements produce sensations. He recommended that sensations, therefore, be admitted to the list of associatively instigatable items. But why did he not say that the movements, themselves, are the associatively instigated items which need recognition? So far as Hunter, himself, is concerned, I suspect that he stopped his analysis one step short because he was then only a moderate behaviorist. But there is no reason why any of us should stop short to-day. Movements, as movements, are widely acknowledged as directly approachable psychological data.

Among the types of associatively instigatable conditions there is one deserving special notice. I refer to the several varieties of inhibition. In many cases the suspension of some particular activity, or the prevention of it, is due simply to a conflict between it and another activity which for one reason or another has greater power. Such a conflict and suppression may be at the muscular level; the posture or movement of one muscle or group of muscles may offer direct physical opposition to the occurrence of some other posture or movement. Conflicts may also occur at the level of cognition and feeling. Inhibition, in the sense of conflict and interference, does not imply the existence of peculiar types of associated items. In order to render such phenomena comprehensible, it is only necessary to remind oneself that associatively instigatable items, such as movements, perceptions, ideas, feelings, and determining tendencies never occur as isolated, individual acts. Because of the constant multiplicity of simultaneous associative instigators, there is a constant mul-

tiplicity of simultaneously instigated items among which interferences may take place.

There are, on the other hand, instances of inhibition that seem to require the admission of a new type of associable condition. General relaxation, natural sleep, hypnoidal sleep, hysterical paralysis and anaesthesia have not been very plausibly explained in terms of the conflict of the positive conditions heretofore enumerated. Yet those states of inhibition give unmistakable evidence of being susceptible to association and to associative instigation. Whether such states of inactivity function as associative instigators is not nearly so clear as the fact that they can be associatively instigated. When sleep is followed by waking at an accustomed hour it is quite likely that the instigator of the waking is some act, or set, over and above the general condition of sleep. The fact that the sleeping organism is never "completely asleep" makes this possibility a most credible one. On the other hand, such states as determining tendencies are hardly positive activities in the ordinary sense of that expression, yet they surely function as associative instigators. This fact would make more plausible the notion of a state of inhibition as an associative instigator.<sup>8</sup>

<sup>8</sup> It should be realized that any classification of inhibition, in the present state of psychological and physiological knowledge, must be of doubtful validity. We are to-day as uncertain of the proper scope of the conception as we are of the worth of the theories that have been advanced to explain the fundamental conditions of decrement. The distinction I have made here between inhibition as a relative matter produced by the fact that a conflict between positive activities comes out in one way rather than in another and the inhibition which involves suppression without conflict is roughly valid. But the inhibition produced by conflict is itself no simple affair and there is a certain danger in classifying all such conflict inhibitions together. It is even more likely that inhibitions not involving direct conflicts are of a number of varieties. Dodge has recently gone over the concept of inhibition and the theories of inhibition and his papers should

## Possibilities of Simplification

Such an inventory of associable items as has here been made is in the interests of accurate and complete analysis of complicated psychological phenomena. It is now fair to ask whether the relatively heterogeneous list of associable states enumerated in this survey can be simplified. The theory that all psychological activities are simply compoundings of reflexes has been sufficiently criticized. Only by unbridled speculation or by ignoring usual definitions can one reduce all perception, ideation, determination, feeling, and moving to reflex units. Another theory that has occasionally been taken very seriously would see in all psychological activity units of sensation, image, and simple feeling. This theory is certainly not applicable beyond the range of the introspective method, and the doctrine of association has no necessary limitation to the domain of the introspectible. A third analytic theory, much more dynamic in its interest, would divide psychological activity into three phases: the cognitive, the affective, and the conative. While these three phases of psychological activity *can* be distinguished, the finality of the distinctions is now very seriously questioned. And, in view of such psychological analysis as is now actually practiced, there is reason to question whether these three categories are all-inclusive. This latter point is well illustrated by the state of affairs in regard to conation. Does conation include movement as well as purely subjective phases of impulsion? Most expositions of the category incline one toward a negative answer. But movement, as movement, is being frankly recognized as a legitimate element mastered by anyone ambitious to argue or to experiment in this domain. (*Psychological Review*, 1926, 33, 1-12, 106-122, 167-187.)

See also by the same author: *Conditions and Consequences of Human Variability* (1931).

ment in the psychological complex, particularly when the analysis is in terms of the conception of association. One may pass innumerable resolutions that the psychologists should content themselves with experiences of moving and intentions to move and leave actual bodily movement to physiologists. The psychologists are not doing so, however, and it is what psychologists actually are doing that should guide the formulation of theory.

Other possibilities of reducing to simpler units the heterogeneous list derived from the present survey are to be seen in the more highly speculative theories such as the synaptic theory of brain or Münsterberg's theory of psychical atoms.<sup>9</sup> But such speculations do not offer any method of actual analysis after the manner of the atomic theory of matter. They simply make the harmless statement that every psychological activity could be reduced to the explosion-like discharges of individual neurones or to the aggregation and disaggregation of psychical atoms, if only we knew enough to make such reductions. In other words, the reductions are of a kind that could be made if they could be made. While there is no doubt that they could, the statement of this fact should not be confused with the provision of a method actually utilizable in analysis.

If one turns away from the elementary units of psychological activity that have been suggested, one is forced to forswear analysis altogether, to arrive at a new element or set of elements, or to enter upon the project of analysis without attempting to state the limits of that analysis. Since he who forswears analysis never really lives up to his resolution but merely analyzes from some other point of view (as is so obviously true with the gestaltists) that possibility can be put aside. As to the hope of discovering new units of psychological activity, my belief is that, if such elementary

<sup>9</sup> *Psychological Review*, 1900, 7, 1-17.



units have not stood out as more or less inevitable limits to associative analysis, they are not likely to be hit upon simply by our taking thought and inventing them. The actual state of affairs would seem to be that we can discern manifold associative connections at many levels of psychological complexity, but what are the simplest discernible, associable units we do not at this time know. There is every reason to believe that associations may be formed between units that are too simple to permit experimental isolation. There is some reason to believe that units of activity more minute than that activity represented by the action of a single set of neurons can be associated. (Witness learning in lower organisms.) But actual analysis needs more substantial guidance than can be had from the hypothetical nature of its hypothetical, limiting terms. Thus, sensory processes, perceptions, thoughts, feelings, determining tendencies and the rest may not be resistant to further analysis, but they have the virtue that they represent types of units actually attainable.

It should be realized that the determination of its limits in the shape of stable, unanalyzable units is only one of the several ideals of a complete analytic system. Even in the recent history of chemistry, the limits of analysis have changed without necessitating a complete rejection of earlier procedures. The cell of general biology is far from setting a limit to analysis and it is far from being a stable unit, yet its usefulness in giving a definite direction to the analysis of organisms is unquestioned. In physiology the processes of digestion, reproduction, circulation are by no means limits of analysis; they are by no means elementary; yet they are capable of giving direction to physiological analysis. It is a familiar fact that psychology, in judging its own analytic procedures, has been obsessed with the idea that an unchanging chemical theory of matter is the model in terms

of which all other analytic systems should be judged. Thus, the theory of association has been attacked on the grounds that its limiting units are either unknown or fictitious. But, on such grounds, most analytic procedures could be overthrown. There must be recognized units toward which analysis proceeds, but since they need not be absolutely irreducible units, they may well be units that analysis actually can reach.

## Chapter III

### SIMULTANEOUS AND SUCCESSIVE ASSOCIATION

#### Some General Considerations

THE condition in which the instigating and instigated items occur in immediate succession has had more attention than the condition in which the two items occur simultaneously.<sup>1</sup> One does not have a long search for the explanation of this preference for the phenomena of successive association. In a chain of acts or states the operation of the associative principles can be discerned without the consideration of items that would naturally be ignored. After the succession of ideas of black, white, snow, arctic, explorer, gun, bear, fur, seal, ship, sea, Europe, politics, one can readily make out that the sequence is associatively determined without considering any of the details of the successive states except the very central fact for which each stands. The notion of successive association is about as comprehensible to the school boy as it is to the professional psychologist. Simultaneous association is a much more difficult notion. In the first place, it requires the ability to abstract from the continuously changing activity a cross section conceived to be independent of temporal progression. Although all scientific points of view require abstraction from complete experience this particular abstraction is a peculiarly difficult one. The study of simultaneous association involves

<sup>1</sup>H. C. Warren, *A History of the Association Psychology* (1921), pp. 273-276.

the noting, not only of that item which at any moment would naturally stand out as the important one, but also of one or more items, that, by virtue of the selectivity of all psychological activity, would naturally tend to be overlooked. Nevertheless, both simultaneous and successive associations are generally recognized to be legitimate conceptions. Any survey of the theory of association must, therefore, look into these conceptions and consider the difficulties and controversial points that they raise.

One possible source of confusion may be mentioned very briefly. The temporal relations considered by the distinction between simultaneous and successive associations are such as to hold in the manifestations of established connections. Simultaneity and immediate succession are also mentioned as conditions important, or even indispensable, for the *establishing* of associative connections. This latter is, of course, the classic law of contiguity. But one may speak of a certain connection between simultaneously or successively occurring acts without reference to the conditions of simultaneity or succession present during the establishment of the connection.

Before entering into the questions particularly pertinent to one or the other of these two types of association—the simultaneous and the successive—it may be well to give further attention to the criticism of association that is likely to be brought to bear in connection with the distinction under discussion. As has been intimated, the notion of connections between simultaneously occurring states is dependent upon a process of conscious abstraction. Psychological events are always moving onward in time and it is only by pretense that they can be stopped. One can also show that successive associations, as they are conceived and described, are artificial. If the flow of activity is actually continuous, which is very generally admitted, who is to separate sharply

the idea of white from the idea of snow which succeeds it? Such a separation can never be more than artificial. Thus it might be shown that either type of grouping is a mere arbitrary device of the scientist and, this being the case, the distinction itself and the general theory that finds such a distinction important are to be disdained.

One must admit that such charges of abstraction are true. After having made such an admission, however, there is no good reason why one should abstain from the distinction between simultaneous and successive association nor from the further description of associations of both types. The criticism implies a certain logical evaluation—an evaluation that would disapprove the abstract and commend the concrete as those characteristics appear in the facts and conceptions of science. But there is no overwhelming evidence in favor of this logical theory. Abstractions may have high or low logical values according to the skill with which they are constructed. Abstractions are simply efforts at making experience intelligible and our knowledge of it specific and precise. In the case of the abstract conceptions under discussion, they may or may not be accurate, or true, or useful, but it is quite as irrelevant to discard them because they are abstract as it is to discard Euclidian geometry or the general theory of association for that reason.

### Successive Association

Turning our attention now more particularly toward the successive type of association, we may recognize as an initial problem the relationship between succession as associatively determined and succession as a feature of stimuli. This distinction is hedged about with controversies and ambiguities that cannot be entirely ignored. In his chapter on "Organization under Stimulus and Receptor," Bentley attempts to

describe those successions (as well as other "formations") of psychological events which depend most clearly upon the temporal arrangement of stimuli.<sup>2</sup> The phenomena of rhythm and melody are both used as examples of successions whose organizations depend largely upon the conditions of stimulation. Now, I am not disposed to argue that melody, let us say, is more dependent upon associative conditions of succession than it is upon succession of stimuli. But a certain case can be made for such an argument. Consider the evidence to the effect that in a melodic sequence there is an associative unfolding of motor sets which anticipates to a degree the occurrence of the successive stimulations.<sup>3</sup> If one contrasts the melody remembered and the melody heard, one has a marked difference between the degrees to which stimulus succession and associative succession are involved. The troublesome situation arises when one attempts to disentangle the successivity of stimulation and that of association when both are present at once, as they clearly are in the hearing of a melody.

While clarification here depends somewhat upon a better knowledge of the facts—knowledge, for instance, as to the validity of the motor theory of melody—it depends even more upon a better definition of fundamental conceptions. One notion that is constantly causing trouble is that of the stimulus. Most writers do not trouble to define it at all; some include under it some kind of a philosophical, real world, and still others mean by stimulus any antecedent event in a psychological series. Before the question can be settled as to whether melodic succession is or is not a largely stimulatory succession, there must be a decision as to what is to be included under the conception of the stimulus. If

<sup>2</sup> *The Field of Psychology* (1925), pp. 115 ff.

<sup>3</sup> See W. V. Bingham, *Psychological Monographs* (1910), 12, whole No. 50.

the stimuli involved in the phenomenon of melody are merely the physical features of the total affair, then the *orderly* succession will hardly turn out to be included under the conception of the stimulus. From a purely physical standpoint a tonal series arousing an experience of melody is no more *orderly* than any other sequence of tones. The physical succession is merely a condition of a final outcome. If, on the other hand, the melodic stimulation, as such, be endowed with all of those traits, such as serial unity, which the principles of association are capable of explaining, then there will be little or no room for the application of such principles. As psychologists, we may well take care never to put too many characteristics into the stimulus. Otherwise, we shall have nothing to do psychologically. In most of his moments the musician does well to give objectivity and independence to his conception of the smooth, sweet melody that falls upon our ears. The psychologist does better to ask how a series of merely physical sounds comes to be, for each individual who hears it several times, a smooth, sweet melody. I am not pleading for a purely physical interpretation of the stimulus, though I think that in this case it would be useful. I am pleading merely for a clear definition of what is to be meant by the conception in its application to a given analysis, and I am adding the suggestion that the psychologist is safer to restrict the scope of the conception as much as is practicable.

Thus far, in discussing the determination of succession, I have contrasted determination by stimulus with determination through associative connection. Although there may properly be a postponement of detailed discussion of the point, notice should be taken of the fact that succession may be governed by other primarily psychological principles than those of association. Conceptions of perseveration, fatigue, adaptation, commonly regarded as basic principles of psy-

chological determination, are not to be dismissed as clearly subordinate to association.

Of all questions that arise in connection with the study of associative succession, probably none is more familiar than that which has to do with the continuity of such succession. It is often assumed that in so far as a sequence is a single integrated series, it must be absolutely continuous. Item *A* is not capable of instigating item *B* if there intervenes in the succession item *X* which is not associatively connected with both *A* and *B*. For earlier experimentalists the interesting question had to do with whether associative successions with apparent breaks were to be accounted for in terms of some mediating, unconscious item. Some concluded that sufficiently keen introspection would always find a conscious factor bridging the apparent gap, while others concluded that the mediating function might be served by a process inaccessible to introspection. It seems to me that the evidence is overwhelmingly in favor of the acceptance of unconscious mediating factors. Such study as has been given to the organization of complicated acts of skill offers convincing evidence that a large number of the most indispensable cues are not only normally unconscious, but resistant to the most skillful introspective scrutiny. Then there are perceptual organizations with their tendency toward unconscious connections. One of the strongest bits of evidence comes from muscle reading and similar "automatic" activities where the attempt to become conscious of vital associative links is very apt to break up the whole organization. That investigators could debate the issue as long as they did was due to the fact that they confined their attention to the successions of ideational processes.

Lately there has appeared what seems to me a much more important possibility relating to the continuity of associative connections. The notion that a given associative succession



must be really continuous, even if not apparently so, has itself come in for direct questioning. Carr suggests the possibility of an actually broken associative succession on the basis of the fact that rats were able to master his alternation problem.<sup>4</sup> The animals were released from a food box a number of times each day. Alternately they were allowed to reënter the food box by following the right pathway and the left pathway. Now, between running right and running left, there was invariably interposed a period in the food box. Thus, the presumable sequences were: run right, eat, run left; or run left, eat, run right. If eating in the same food box is equivalent in the two cases, then this activity is not the instigator of the choice of pathway, because it could not be the instigator of running both right and left. The obvious instigator of running right is running left, and of running left running right, but the members of these pairs are separated in time by an item which, because of its position between the members of one pair, cannot be the associative link between the members of another pair. Either the facts of the case are not what they seem, or else associative successions can be broken temporally and yet hold together functionally. There are a number of possibilities for making the facts not what they seem. There may, of course, be a continuation of postural remnants from the running left through the eating that immediately follows, and likewise in the case of running right. No evidence of such sets was observable, however, and other suggested methods of explaining away the interrupted associative succession fail to attain plausibility in the light of the behavior of the animals. So, while the actually interrupted associative succession has not been finally proved, the facts certainly point in that direction.

There are certain considerations that make the notion of

<sup>4</sup> *Psychological Review*, 1919, 26, 335-353.

interrupted associative succession not so astonishing after all. The conception has been that in such a succession there must be not only a continuity of instigating factors, but also that these factors must be activities, as activities. There is a question as to whether states of activity are the only kinds of states capable of instigating associated acts or states. A determining tendency is picked up with a word of instruction and, as context for a later cue, instigates a thought or a movement. Yet, what is the evidence that the tendency occurs as activity, at the time its influence is felt? The hearing of the directions was activity, but later, when they are obeyed, is it necessary that the earlier activity gets reinstated as activity? Is it not possible simply that different associative biases have been established? If so, there is a real sequence, and an associative sequence, between the hearing of the instructions and the appearance of their influence, but the sequence is, so far as continuity of *activity* is concerned, distinctly interrupted.

The conditioned trace reflex offers further evidence upon the present point. In this case, the unconditioned stimulus is presented, during training, some time after the presentation of the conditioned stimulus. The desired reaction is thus associated with the conditioned stimulation, but it does not occur until the usual time between conditioned and unconditioned stimulation has elapsed. Either the reaction comes to be instigated by a stimulatory activity separated from it in time or else it comes to be instigated by the relatively neutral state correlated with the passage of time.

### Simultaneous Association

The special treatment of simultaneous association may well begin with a question parallel to that with which the subject of successive association was introduced. How is a

line of distinction to be drawn between that simultaneity which is associatively determined and that which is determined by such other factors as the nature of the stimulus? Where one or both simultaneously appearing items are acts of imagining or remembering—acts not closely related to the nature of the immediate stimulation—we can be safe in assuming psychological association to be at least a large influence in the determination of their simultaneity. But, as in associative succession, the greatest confusion arises when we are confronted with groupings of items that are closely related to immediate conditions of stimulation.

One source of error in this situation is the tendency to assume psychological association wherever there is a lack of discrimination between two items of experience or behavior. When the little child sees the whole lamb, are we to assume that head and legs and body and tail of the perceptual ruminant are held together through the operation of the principles of association? Clearly such an assumption is folly, because it involves the reverse assumption that the disparity of these various members of the lamb was an original condition for the baby. Disparity, however, means some definite kind of isolation and it is ridiculous to suppose that the separation of these particular segments was ever a forerunner of their integration. The psychological individual no more makes his start as a perfect analyzer whose analyses must be overcome, than he does as an equally perfect synthesizer.

We may lay it down as a safe rule that two or more simultaneously occurring items are not to be called associated unless it can be shown that a present condition of connection has been preceded by a condition in which such connection was lacking, but in which definite discrimination may also have been lacking. It is a clever remark that attention (or perception) is at once analytic and synthetic; in

looking at the picture, we hold its parts together and at the same time distinguish nicely between the picture and the wall. Yet this is not psychology at all; there is little reason to believe that the associated parts were ever separate or that the distinguished parts were ever together. There are relevant applications of the theory of association to looking at a picture on the wall, but they can hardly be made through any common sense or logical description of the situation. Only a genetic study of this complex activity will tell us what are the items that have become connected.

The conception of simultaneous association, like that of successive association, can be clear only if our conception of the stimulus be clear. Although the human individual's simultaneous patterns of psychological activity are originally in a largely chaotic condition, the stimulus is assumed to be present with all of its peculiar modes of variation. When these modes are as definite as they are in the case of the physical stimulus, psychological investigation has a valuable set of cues. While it is not true that any items which are two physically may be said to be associated if they are reacted to as one psychologically, nevertheless the known physical twoness gives a hint as to what *may* have happened associatively and the possibility offers a definite problem. Consider the unity present in the hearing of two tones of a consonant interval. The stimulus contains two vibration frequencies that are clearly distinct, yet the perception has a unity that must be explained. The double character of the stimulation *suggests* that two sense processes have become associated, but it does not prove that this is true. The "beat" theory might say that psychologically the two processes had never been distinct. If this be true, they are not now to be thought of as associated. The "genetic" theory, of course, would say that the present unity has been attained out of a more primitive type of unintegrated psychological respon-

siveness. This illustration cannot long be dwelt upon without getting us off the track and into its own complexities, but the facts I have in mind are well illustrated by it; the notion of the stimulus as a clearly conceived given in a psychological situation furnishes, not a final answer in regard to what are associated, but a clue as to what probably are, or may be, held together through the operation of the psychological principle.

On an earlier page (46) the point was made that sensory processes are not among those conditions that are associatively instigatable. Yet the possibility of something like simultaneous association of sensory processes was admitted in the last paragraph, even though there was an insistence upon the difficulty of determining such associations. There is, however, no real exception or contradiction involved. The union of two or more sensory processes in perception is a function of the fact that we have gone beyond mere sensory process. If the sensory processes were directly associated, it would have to be shown that one is instigator and the other instigated. Both, of course, are aroused by the simultaneous play of stimuli upon the sensory mechanisms. Not until the sensory processes have actually instigated an act of perception or interpretation can we properly speak of associated sensory processes.

As a final point regarding simultaneous association, something should be said about the relation of this type of association to the insight of the gestalt theory. Writers of this school studiously ignore the established principles of association formation on the ground that these principles are largely in error. If one leave out of account the question of origin, one finds that the states and activities with which these writers are concerned when they speak of insight have a high degree of resemblance to those described by associationists as the results of association formation. It has seemed

to me that what the gestaltist means by insight is about what the associationist means by simultaneous association. In both cases a complex of simultaneous stimuli produces a state in which the several psychological results of these stimuli enter into a simultaneous organization.

Now if insight be approximately a state of simultaneous association (putting aside all questions of origin), there are certain assumptions about this state made by the gestalt theory that are of great importance for any thorough theory of association. When the gestaltist sees an individual (human or infra-human) pause in the midst of a problematic situation and then suddenly enter upon an appropriate pattern of action, he says that the pause was the moment of insight and that this insight explains the appropriateness and integration of the acts that followed. There is in this description and hypothesis an assumption that the gestaltists do not, to my knowledge, question, but which certainly should be questioned. It is assumed that if configuration or organization be present in a momentary state—as in insight—this fact constitutes an explanation for the configuration or organization of a series of acts that afterwards unfolds. Translating the matter into terms of association, the assumption is made that if a succession of integrated acts be preceded by a simultaneous association, the latter will automatically account for the former. Proof that such a perfect relationship exists between the organization of simultaneous states and that of succession would be of the first importance, but without proof the relationship is a precarious assumption. In the gestalt theory, which has done much to crystallize this problem, there is no proof. The observed fact is the organization of a series of acts following a momentary period during which nothing of importance is observed to happen. Advocates of this theory are able to fill this preceding period with insight or with simulta-

neous organization, only because of what has been assumed to be an established relationship between simultaneous and successive organizations.

It is not at all my purpose to argue that there are no fundamental principles governing the relationship between simultaneous and successive organizations. But it is far from a well-based assumption that we can count upon one type of organization automatically producing the other. Undoubtedly the adequate cue for many organized successions of psychological states is a simultaneous organization, but we are hardly able to say what are the conditions under which this relationship holds. It seems to me that there are enough cases where simultaneous organization fails to instigate an organized succession to warrant a factual investigation of relationships between organizations of the two types. It might be added that there are also many organized successions preceded by disorganized, rather than by organized, simultaneous states.

This whole problem reminds one of the earlier discussions concerning the ideational control of movement. The first assumption was that, if only a clear idea (image, sometimes) of the desired movement could be obtained, then, Presto! the movement would take place. Especially at the hands of Thorndike<sup>5</sup> this assumption received wholesome criticism. And now, having lost our assumption about the relationship between ideation and movement, we have left an empirical problem as to what relationships do hold in this matter and under what conditions they hold. The more the assumption underlying the use of the notion of insight is examined, the more clearly it will be seen that the relation between simultaneous and successive organization is a matter for empirical investigation rather than for easy hypothesis. This is certainly true from the standpoint of association.

<sup>5</sup> *Psychological Review*, 1913, 20, 91-106.

## Chapter IV

### THE "LAWS" OF ASSOCIATION: CONTIGUITY AND ASSIMILATION

#### Preliminary Considerations

THE "laws of association" usually mean simply those factors that determine the strength of associative connection. If we were agreed as to the list of such factors we might then state the general condition of affairs as follows:

*The strength of any associative connection is a function of the conditions of contiguity, frequency, vividness, and so on, obtaining at the time the connection was formed.*

Actually there is much disagreement as to the exact list of factors determining associative strength.

It is common practice to divide the laws of association into two classes, the qualitative and the quantitative.<sup>1</sup> There is general agreement that one of the qualitative laws is that of contiguity. Concerning the laws of similarity and contrast there has been a great deal of controversy. At the present time the tendency to place these laws in the same class with that of contiguity is not at all strong. Carr has recently suggested that the law of assimilation is the second "explanatory" (qualitative?) principle.<sup>2</sup> This suggestion

<sup>1</sup> H. C. Warren, *A History of the Association Psychology* (1921), pp. 282-287.

<sup>2</sup> *Psychology* (1925), p. 103.



might possibly meet the approval of writers like Thorndike and Woodworth who have paid considerable attention to such a law. Among the laws commonly regarded as quantitative we have those of primacy, recency, frequency, vividness, duration, permanent tendencies, temporary set.

Before going forward to a study of the various individual laws of association, a question may fairly be raised as to the precise meaning of the distinction between the qualitative and the quantitative laws. Conceivably the distinction might be between a more fundamental and a less fundamental group of principles. The implication might be that such a principle as that of contiguity is assumed in the statement of the less fundamental principles, though it can, itself, be stated without any reference to matters of frequency or duration or set. A second possibility is that the qualitative laws deal with conditions that are either present or absent and that, with them present, there can be association, while, with them absent, there cannot be association. In other words, such laws may be all-or-none laws contrasted with those representing the variable influence of some variable factor. A third possibility regarding this distinction is that it refers simply to two stages of scientific achievement—a more primitive stage of merely verbal description and a more advanced stage of mathematical or, at least graphic, formulation. And, finally, the division might be simply between factors having a stronger influence and those having a weaker influence in determining association. Thus, the conception might be that predictions from a knowledge of contiguity will be right ten times as often as predictions from a knowledge of frequency.

Whether authors who have distinguished between qualitative and quantitative laws of association have been justified in so doing can hardly be decided without reference to which of the several possible meanings of this distinction they have had in mind. Unfortunately it has not been the practice to

say what is meant by *qualitative* and by *quantitative* in this connection. It has apparently been assumed that this difference is too obvious to need definition. There is thus great difficulty in dealing accurately with the matter. In so far as one can judge from implication, I believe that most authors have had in mind a distinction between more and less fundamental principles—the qualitative principles being assumed to be more fundamental than the quantitative ones. But now that the general question has been opened we shall do better to postpone its final settlement (if such be possible) and to consider it later in regard to individual laws of association. And in the case of these laws it will be relevant to ask, not simply whether the law is qualitative or quantitative, but whether it is fundamental, whether it states an absolutely indispensable condition of association, whether its statement can actually or conceivably be made mathematical or graphic, and whether the factor with which it deals should presumably have a great or a small weighting in the prediction of associative strength.

Although the laws of association have applied largely to the conditions under which associations are formed, we occasionally find mixed among such laws principles that have more to do with retention or associative reinstatement than they do with associative fixation or establishment. While the fixations of association can be examined only through the operation of reinstatement, which in turn implies retention, a very useful distinction can be made between the factors affecting fixation and those affecting retention and reinstatement. And where the same factors affect both, as in the case of frequency, it is often possible to distinguish between the two effects.

Two laws that should at once be eliminated from the group dealing primarily with fixation are those of primacy and recency. Neither factor can have any meaning until

after a fixation process has taken place. The law of primacy is concerned with the activities that intervene between fixation and reinstatement. The law of recency refers to the influence of the length of retention of a fixated connection upon its readiness for reinstatement.

There is another set of principles that should be kept separate from those principles of associative fixation that are now under discussion. I have in mind the cataloguing of stimuli according to the likelihood of their arousing associable acts. Cattell, Kraepelin, Jung and others have classified those relationships between associated words which are frequently observed in the free association experiment.<sup>3</sup> To a great extent (though, by no means always), these relationships are logical and grammatical; therefore they can well be regarded as relations between stimuli rather than as relations between psychological processes. Such classification is useful and informing. There are certain predictions that can be made about the probability of association occurring between two processes, if we know the characteristics of their stimuli. The chief objection to existing classifications is that they do not go far enough and that their rubrics are often badly mixed. As Carr, for one, has pointed out, there are organic, social and physical relations between stimuli which point toward association formation.<sup>4</sup> Such relations should be classified. But in the extension of present classification there should be an effort to keep clear as to the various rubrics of classification.

This classification of stimuli from the standpoint of association is readily confused with the more direct treatment of the conditions of fixation. Such a relationship as contiguity holds for stimuli as well as for psychological processes and

<sup>3</sup> H. C. Warren, *A History of the Association Psychology* (1921), pp 247-257.

<sup>4</sup> *Psychology* (1925), pp. 102-106. )

from either standpoint certain prophecies of association can be made. As Woodworth shows,<sup>5</sup> however, prediction of association on the basis of mere contiguity of stimuli is very uncertain. As applied to psychological processes the relation becomes a more important factor in association, and probably most psychologists would be disposed to mean by the *law of contiguity* the *law of temporal contiguity of psychological processes*.

The factors of similarity and contrast, which have often been placed beside contiguity in their importance, have usually had a bearing merely upon a logical classification of readily and frequently associated words. Something more may be meant in the case of *association by similarity* (as will be seen in a subsequent discussion of assimilation), but the expression is better set aside for use in connection with the logical classification of stimuli.

Of course, it should be understood that the classification of stimuli from the point of view of their likelihood of affecting the operation of association is itself a formulation of certain of the conditions of associative fixation. If we put a kitten and a puppy in the nursery we may be sure that when the children come in there will straightway be set up in them certain associations between the idea of one animal and that of the other. Without concerning ourselves about the details of fixation, when it actually occurs, we can predict that a connection will be made. Our prediction is based upon the physical contiguity of the animals and upon their similarities and differences. Nevertheless, predictions of this kind are based upon facts which, while they are related to the issue, are indirectly related to it. There are always other intervening facts which, if they can be reached, will make prediction more sure. Two similar stimuli are, by virtue of that similarity, the more likely to arouse processes that will be-

<sup>5</sup> *Psychology* (1921), pp. 396-398.

come psychologically associated. However, it is mainly by going on from stimuli to psychological processes that we find conditions which, in a given instance, substantially increase our understanding of the degree of association attained. The enumeration of the kinds of organic, physical, social, grammatical, and logical relations likely to result ultimately in psychological associations is a practical procedure useful mainly where immediate results are more important than exhaustive comprehension. That certain kinds of behavior are likely to become associated with the presence of over-solicitous parents is useful information. It may aid greatly in the raising of children. But it sheds little light upon the primary principles of fixation.

### The Law of Contiguity <sup>6</sup>

Reference has already been made to the law of contiguity. The greater number of writers considers this law of fundamental importance. It is commonly said to be of an order different from that of the laws of frequency, duration, intensity, individual constitution, and temporary set. By many psychologists it is regarded as the one qualitative law which states the very essence of the principle of associative fixation. Although precise statements of what is meant by *conditioning* are infrequent, there is reason to believe that the term is ordinarily used in reference to associative fixation dominated by the contiguity of the connected processes. The case is clear in a definition by Smith and Guthrie: <sup>7</sup> "*When a response is elicited by a new stimulus, because of the fact*

<sup>6</sup> Compare the following treatment of the law of contiguity with that of Carr, "The Laws of Association," *Psychological Review*, 1931, 38, 212-228. See also H. Ebbinghaus, *Memory* (1885, tr., 1913, by Ruger), Chapter IX.

<sup>7</sup> *General Psychology* (1921), p. 88.

*that the new stimulus has occurred along with the old, it is called a conditioned response."*

There is no reason to question the claims that temporally contiguous processes have a certain tendency to become associated which they would not have if they were less contiguous. But as soon as that point is passed we come upon trouble enough to satisfy the most cantankerous of us.

Let us put down a concise statement of the law: *The fact that two psychological processes occur together in time or in immediate succession increases the probability that an associative connection between them will develop—that one process will become the associative instigator of the other.* This statement has not been found in the literature in exactly its present form. Its wording attempts to keep it in accord with earlier phases of the present discussion without essentially altering the traditional law or its modern equivalents. Whereas the law is often given as though applicable only to ideas or only to sensations and ideas, I have used the general term, *psychological processes*, the content of which has already had discussion sufficient for present purposes (Chapter II). I have stated the law as though it were one of temporal contiguity and not one of temporal *and* spacial contiguity. Spacial contiguity is a physical condition which often leads to temporal contiguity of processes, but it is not itself as immediate a condition of association as is temporal contiguity. This point is tacitly accepted by many modern associationists.

Now for controversy! Although it would be freely granted that the contiguity of psychological processes is capable of affecting the degree to which they may become associated,<sup>8</sup>

<sup>8</sup> There are a number of common criteria of degree of association or associative strength. Probability of the "correct" activity being instigated is one such criterion. Fidelity of the instigated activity to some norm, such as its nature when observed in the fixation process, is another criterion.

a question might be raised as to whether this factor has a unique order of fundamentality. Does every other factor operate only as a modifier of the fundamental influence of contiguity?

It should be kept in mind that mere coincidence in time or mere immediacy of temporal succession will not insure the establishment of a demonstrable association between two psychological processes. Thus, even if it could be shown that there cannot be association without contiguity, the presence of the factor of contiguity is not enough to insure association. Such a factor as frequency, which is often thought of as secondary, may have to be taken into account before we can predict the result of a given condition of contiguity. If frequency be called a secondary factor because it is influential only under proper conditions of contiguity, is it not equally true that contiguity should be relegated to the same level because its influence is so dependent upon concomitant conditions of frequency? I am not trying to show that contiguity is an unimportant factor; I am simply arguing that there is a real question as to whether it is not dependent upon other factors—generally thought of as secondary—in the same way in which they are dependent upon it.

The distinction between the factor of contiguity and the so-called quantitative factors may, as I have indicated, imply a difference between a factor which, with its peculiar effect, is either present or absent and factors which vary continuously both in their own magnitudes and in the magnitudes of their effects. Just why psychologists should have assumed, or talked as though they assumed, the qualitative all-or-none character of contiguity is difficult to understand. Warren seems to accept the contention that the factor of

A third is the speed of instigation. The very general laws of association here under consideration apply to all of these meanings of degree of association. Compare with Thorndike, *Educational Psychology* (1913), II, 2.

contiguity is qualitative. Yet he states the law of that factor in these terms: "A sensation or idea tends to recall other experiences which formerly occurred in close proximity to it."<sup>9</sup> But how close is close? The language of some authors suggests that actual temporal overlapping, or at any rate immediate succession, is required. But contiguity is after all a continuous variable and the supposition that we get association only with a zero interval between associated processes is not exactly what one would expect. If one can for a moment forget the customary discussions of the question, it would seem as though we ought to get varying degrees of association with varying degrees of contiguity. This is particularly true inasmuch as association is clearly not an all-or-none matter. Since association can be produced in various degrees, we may ask what degree of association it is that is associated with that crucial zero time-interval. On largely logical grounds it thus appears that the law of contiguity is a functional relation between two continuous variables—contiguity on the one hand and degree of association on the other. And, of course, if this fits the facts as they are, there is no really good reason for continuing to speak of the law of contiguity as *qualitative*.

That there is actually a continuous relation between degree of contiguity and degree of association is difficult to demonstrate experimentally because of the constant possibility that association between two temporally separate items is mediated by some intervening item. The method utilized by Carr in his alternation problem attempts to get around the difficulty by filling the interval between one associated pair, such as R—L, with the same psychological process as that which fills the interval between such a pair as L—R. (See p. 59).

<sup>9</sup> *A History of the Association Psychology* (1921), p. 283.



Yarborough,<sup>10</sup> one of Carr's students, has reported a systematic investigation of this problem. After his rats had acquired a turning reaction in response to an induction shock, he transferred this behavior to an auditory or visual stimulation. During the formation of the new association, the time-interval between auditory or visual stimulation and the turning behavior was varied by varying the temporal position of the shock. For association in the forward direction—when the auditory stimulation was given before the occurrence of the response, there was little difference between the condition of simultaneity and that of a one-second interval. Association was possible for the longest interval employed, six seconds, though there was little difference in efficiency between the two- and the six-second conditions. Association in the backward direction proved possible and, where the time-interval was zero, hardly more difficult than in the positive direction. With an interval of as much as one second, however, the backward association was exceedingly difficult, if not impossible. If these findings were put into the form of a graph we should have a function of the general type illustrated by the accompanying figure.

This function is exceedingly interesting and it does not seem at all unlikely that inflections of these types, though not always at these particular temporal points, will ultimately be shown to characterize analogous functions obtained under a wide variety of detailed conditions.

It would be foolish to pretend that Yarborough's experiment is open only to the interpretation that has here been given it. There is the possibility that an animal, after hearing a sound as he entered a pathway, did not continue to behave as though he had not heard the sound. In other words, even here there is the possibility that the associations bridging more than the zero interval were so mediated by

<sup>10</sup> *Psychological Monographs* (1921), 30, No. 135.

intervening activities that the actually associated items were never separated in time. This is a valid objection to most attempts to get at the influence of time-interval in the case

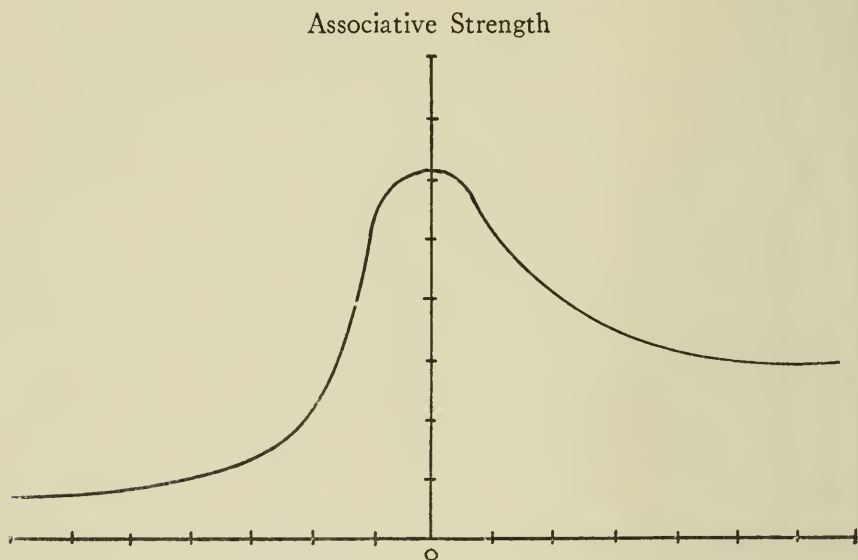


FIG. 1

The General Form of the Law of Contiguity or Time-Interval

of human memorization, because of the ever-present opportunity for ideational rehearsal. As a matter of fact, I suspect that the only absolute method of eliminating the possibility of associative mediation is by killing the animal after the occurrence of the first item and then bringing him to life just before the occurrence of the second item. But so long as the argument for the all-or-none character of the influence of contiguity has to fall back upon the supposition of undetectable mediators it is in a weak position. Why should not the burden of proof be assumed by those who insist that there is always a mediator, even when they are unable to

demonstrate it, rather than by those who admit no mediation of this sort unless there is positive evidence of its existence. To return for a moment to Yarborough's important experiment, this point should be given some emphasis: The immediate succession of the hearing of the signal and the reversal of pathway after the formation of the association would indicate that the principal connection formed was between these two rather than that there were more fundamental connections between these terms and one or more intermediate processes. The evidence for a direct association between temporally separated items is better here than in the case of the trace reflex, where the time-interval present in fixation is still present in the manifestation of the connection. The latter result may well mean that psychological processes taking place during the time-interval are crucial links in an associative chain. The conditioned reflex technique nevertheless is the most promising for the extensive investigation of the influence of time-interval.

The point might be made that contiguity implies coincidence, overlapping, or absolutely immediate succession and that, therefore, the relation of time-interval to degree of association cannot be included within the law of contiguity. The emptiness of this argument should be evident. If the relation at issue is actually a continuous relation between two continuous variables, as there seems to be good reason to believe, perhaps it would be better to speak of the law of time-interval or the law of proximity. My own feeling is that it would be unwise to do so, because we are still dealing with the relationship which casual observation puts down as the law of contiguity. That this relationship should now appear to be continuous in a way not formerly suspected is only a discovery about the details of the long-recognized principle.

There is involved in the full development of the law of con-

tiguity the oft-debated issue as to the possibility of backward association. To place the problem in the present discussion, it might be said that we are dealing now with the case where the presentation of instigating and instigated items have a temporal separation of minus value, which would be the natural graphic manner of expressing that reversal of order in relation to those cases where there is temporal coincidence, overlapping, immediate succession, and various degrees of positive separation, that is, separation in which the instigating item precedes that which is to become instigated by it. (As in the figure on page 76.) Many authors have felt a great antipathy for the notion that an association may be formed which will operate in a backward direction.<sup>11</sup> One argument is that the synapse, which is alleged to be the anatomical seat of varying neural resistance, is irreversible. Another argument is that, even where backward association has been demonstrated, as in the Ebbinghaus experiment with reversed lists of syllables, it has not been clearly established that the subject, in reading the lists in original order, avoided a certain amount of mental backtracking. The second argument is made less important by the fact that Yarborough established a reversed association in his animals. That he was able to do so only with immediate succession in presentation does not destroy the value of his results, though it would, of course, be important to set up a reversed association in animals under conditions separating the associated items by a pronounced interval. Although there are certainly grounds for skepticism as to the importance of backward association in the usual integrations of psychological activities, I believe that there is enough evidence to justify us in saying that they do occur. If we reject the mere possibility of such associations we are assuming that, as we

<sup>11</sup> Recent articles on Memory in the *Psychological Bulletin* will introduce the reader to the literature on reversed association.

reduce the time-interval between associated items, we continue to get a high degree of associative connection up to complete coincidence or perhaps through the region where there is actual overlapping, but that, below such a point, there is a sudden drop to a zero or nearly zero degree of association. The safe policy would be to retain enough interest in the possibility of backward association in given experimental situations to study the effects of the minus as well as of the plus intervals between associated items.

While logical considerations, and also the small amount of experimental evidence at hand would seem to point to a continuous, rather than to an all-or-none, relationship between time-interval and degree of resulting association, the contention could readily be made that the relationship is still essentially qualitative inasmuch as we are quite unable to express the law algebraically or graphically. We cannot present a single empirical function which is the general law. Now, as a matter of fact, it is not upon any such basis that the law of contiguity has been held up as qualitative. This particular contention is, nevertheless, such a natural one that it well may be dealt with.

Here we have a pair of continuous variables—time-interval and degree of association—and a relationship between them. We know that in a general way greater time-intervals are associated with lesser degrees of association than are the smaller intervals. There is evidence that increasing the interval in the minus direction, with reversal of the terms, brings a much sharper drop in degree of association than does increasing the interval in the opposite direction. More than this we do not know, except as we desert the general relation and go into particular instances. Is the indefiniteness of knowledge and opinion in this matter ground for concluding that the principle involved is qualitative rather than quantitative? We are confronted with a nice problem in sci-

entific method and terminology. Since there is no provision for a realm between quantitateness and qualitateness, one of these must be whatever the other is not. If a relation cannot be quantitative unless it be actually reducible to mathematical terms, then such a one as has been under discussion would be qualitative. It would seem to me, however, that qualitateness is an affair of more than negative character—that it means more than lack of precise numerical expression. Does it not imply actual discontinuity rather than mere lack of perfectly demonstrable continuity? If so, a principle should not be set down as qualitative unless there be some actual evidence that it is, or with additional knowledge seems likely to turn out to be, discontinuous. But all available evidence, I believe, points toward continuity in the case of the law of contiguity.

This point has more than dialectical importance. If the law of contiguity be regarded as probably quantitative, there is some incentive for investigating the time-interval—association relationship wherever it can conveniently be done. The fact that there have been so few experimental investigations of the relationship is due more than anything else to the fact that the law has been conceived as qualitative. With contiguity, association is supposed to occur; without it, association is supposed not to occur. If such be one's belief, there is nothing to investigate. Especially if the belief be thorough, as it usually seems to be, there is not even an interest in attempting to give experimental justification to the belief. On the other hand, a quantitative hypothesis regarding the precise nature of the law of contiguity offers a live experimental program.

A final possible reason for separating the law of contiguity (and sometimes certain other laws) from the so-called secondary laws is that this law is somehow more important. To make the question more specific we may turn again to

a comparison of contiguity and frequency, since the latter is perhaps the most definite of the so-called secondary factors. Is there any evidence that in the prediction of associative connections, contiguity should be given more weight than frequency? If there is, I do not know its nature. But I can think of a good reason why the relative importance of the two factors is difficult and probably impossible to evaluate. There are no common units in terms of which the two can be measured. How many times is how contiguous? In the workaday world extremely rough comparisons may be made between the practical cost of so much contiguity on the one hand and so much frequency on the other. On such a basis one or the other might turn out to be the more important factor in the achievement of associative connections. It is, of course, plain to see that such comparisons are to be avoided in scientific formulations. And if we avoid them, we are left with no basis for saying that a given amount of contiguity is more effective than a given amount of frequency or more effective than given amounts of any of the other acknowledged factors of association. One well may ask why there should be a desire to guess at the relative importance of factors of association. What is needed is a large amount of experimental investigation of all of them.

As the other laws of association are brought under consideration, it will not be necessary to examine in such detail the question as to their being qualitative or quantitative in one sense or another. While I shall touch upon the general problem again from time to time, the discussion in connection with the law of contiguity can be applied with little effort to the other laws.

### The Law of Assimilation

The law of assimilation is thus stated by Thorndike: "*To any new situation man responds as he would to some situa-*

tion like it, or like some element of it. In default of any bonds with it itself, bonds that he has acquired with situations resembling it, act.”<sup>12</sup> When the law is stated in this way it is desirable that it be distinguished from what is more commonly thought of as association by similarity. In the latter case association is considered to be a function of the similarity between the terms associated. From the point of view earlier accepted in the present essay, such association by similarity refers to classification of stimuli in the light of their probable effects on association (p. 70). In the case of assimilation, on the other hand, the directly associated items are not especially similar. The similarity obtains between the conditions of stimulation associated with the same response.<sup>13</sup>

We may well pass over the fact that Thorndike states the law of assimilation in terms of *stimulus* and *response* and without reference to other possible pairs of associated activities. Such comments as would be relevant on this matter are readily inferable from the earlier handling of the question as to whether all associative connections are to be regarded as connections between stimuli and responses (p. 37).

Putting aside this very general question, we may inquire about the correspondence of this law with the facts. Among those who are at all sympathetic with the theory of association perhaps the majority would agree that an activity which comes, associatively, to be instigatable by a certain other activity will also be instigatable by activities resembling that other activity. This is not, however, a point to be accepted without comment. Granted that *B* comes to be instigatable by *A*, we may be sure that *B* has also, to some

<sup>12</sup> *Educational Psychology*, 1913, II, 28.

<sup>13</sup> Woodworth (*Psychology*, 1921, pp. 406-408) has an excellent discussion of this distinction.



degree, become instigatable by activities other than *A* and other than those identical with *A*. In fact this much is necessarily true on logical grounds. As Hollingworth says: ". . ., 'the same event' can mean only 'another event in the same class'; and a class is a series that is not fully constituted until all the particulars comprising it have occurred."<sup>14</sup> While, as the same author brings out, this does not preclude the possibility of calling two events the same, careful thinkers will avoid assuming that they ever have any dealings with absolute identity. Since this is the case, the admission that associations established at one time are manifested at later times requires, as corollary, the admission that more connections are formed at the time of fixation than are actually observable during fixation. But the claim that this spread, which all associative connections imply, is a function of the degree of similarity among the instigators is an additional point that will have to be examined on its own account.

No one would be likely to deny that within a limited range assimilation is a function of what is commonly meant by "similarity." I say "good morning" to all the men I meet on the country road, but I do not say it to the trees and fence posts. The instigators of my morning greeting are more like each other in most respects than are any of the trees and posts like any of those instigators. It is unfortunate that we cannot stop here. But as Thorndike himself tells us, the likeness upon which assimilation is conditioned . . . "may or may not be such a likeness as would lead the man to affirm likeness in the course of a logical or scientific consideration of *A* and *B*. For example, diamonds and coal-dust are much alike to the scientific consideration of a chemist, but it is unlikely that a person who had never seen a diamond would call it coal-dust as a result of the law of

<sup>14</sup> *Psychology: Its Facts and Principles*, p. 35.

analogy”<sup>15</sup> (assimilation). What, then, is the kind of similarity of which the scope of assimilation is a function? Thorndike suggests that: “‘For situation *A* to *be like* situation *B*’ must be taken to mean, in this case, ‘for *A* to arouse in part the same action in the man’s neurones as *B* would.’” This hypothesis is so general that there is almost surely some truth in it, but its scientific importance may be questioned. It gives no cue by means of which we can in the future detect bases for assimilations that otherwise would not have been suspected. In the main, recent neurological evidence regarding localization of function has emphasized the difficulty of predicting from such an hypothesis. The notion of neural similarity also has the disadvantage that it seems to be so final. If all cases of assimilation, where there is apparent lack of similarity in the observable items, rest upon unobservable neural similarities, there is little more that can be done about the matter. While a certain challenge to verification or criticism is offered the experimental neurologist, there is nothing for the psychologist to do but state the hypothesis and go his way.

There is far too much to be learned at the psychological level for us to make much of the easy but highly indefinite hypothesis of neural similarity. What is required is a study of the assimilative effects where the instigative conditions vary from each other in some unequivocal manner.<sup>16</sup> One reason why more systematic studies have not been made is, as I have said, the apparent finality of the neural hypothesis. Another reason is that the complexities of the problem have been hidden under the word *similarity*, one of the thickest fogs that ever obscured a psychological issue. As one looks

<sup>15</sup> *Op. cit.*, p. 29.

<sup>16</sup> See the recent work of K. S. Yum, “An Experimental Test of the Law of Assimilation,” *Journal of Experimental Psychology*, 1931, 14, 68-82.

over the innumerable transfer experiments one should be struck by the fact that there are a great many discernible relationships between psychological items of which tendencies toward assimilation may be functions. To say that the differences among these relations should be forgotten in face of the fact that all of them are types of similarity is scientifically vicious. The term *similarity* has such infinite scope and so little specificity that, of course, it can be applied to any relation. Stimulation of the right eye and stimulation of the left are similar; the appearance of one set of syllables and the appearance of another set are similar; but the assumption that these two pairs of similars manifest essentially the same type of relationship is to state what is too obvious to need stating and to slip over that which needs, not so much stating, as investigating.

G. E. Müller's well known ~~law~~ of associative inhibition ~~is~~ a special instance of the more general law before us. According to the Müller principle, an earlier association between *A* and *B* typically renders more difficult the formation of an association between *A* and *X*. Kline, however, has shown experimentally that the presence of an item in one associative connection does not necessarily interfere with its entrance into a new connection. Among other things, his subjects associated the names of states with the names of false capitals. Because of certain previous measurements made of the association in these subjects, between the names of these states and the names of their actual capitals, it was possible to determine the influence of the earlier association upon those experimentally induced. Kline found that the presence of a state name in well-established association with its correct capital rendered relatively easy the association of that state name with the name of a false capital. Interference with the experimental association was present in greatest degree when the association between state and real capital

was only fairly well established. Kline states a case from practical affairs that illustrates the same point. He tells how post office mail sorters very readily form new habits of sorting providing their earlier habits are thoroughly established. Interference is most prevalent where forerunning habits are still loosely organized.<sup>17</sup> In other words, we are brought back to the old adage: "A little knowledge is a dangerous thing."

It is, of course, the chief importance of Kline's experiment that associative inhibition and its partner, associative facilitation, are not separate and unique phenomena. They are rather manifestations of a single general law—the law of assimilation.

Let us return for a moment to the general statement of the law of assimilation. As a result of the discussion up to this point I should state the law in some such fashion as this:

*Whenever an associative connection is so established that an activity, A, becomes capable of instigating an activity, B, activities other than A also undergo an increase or decrease in their capacity to instigate B.*

This bare statement is so fundamental that it can, as we have seen, be justified upon purely logical grounds. But something more is required as an excuse for taking the trouble to state the principle. The statement is worth making for the reason that the instigative power of *A* is taken on by other activities which not only are not identical with *A*, but which would not, from a great many points of view, even be classed with *A*. This consideration brings us face to face with the question as to when and in what degree assimilation can be expected. Here we have an inexhaustible stock

<sup>17</sup> "An Experimental Study of Associative Inhibition," *Journal of Experimental Psychology*, 1921, 4, 270-299.

of experimental problems most of which have hardly been touched. The Pavlov school has done some work on associational spread over a series of tones and over several loci of tactile stimulation.<sup>18</sup> Much more of this definite character should be done. The ideal should be to discover as many as possible of the variables of stimulation and to establish the functional relations between differences as marked off within those variables and differences in the degree of the assimilative effects.

In one sense of that distinction, some of the principles of operation of the law of assimilation will be quantitative and some will be qualitative. Where an association is established between a tone of given pitch and a given activity, there is probably a continuous variation of degree of assimilation over a wide range of tones. Where the association is established between the stimulation of a spot on the right side of the body and a certain activity, and assimilation of the stimulation of the corresponding spot on the other side of the body is measured, the principle involved will, of course, be qualitative.

The complexity of the detailed conditions under which the law of assimilation operates is increased by the fact that these conditions extend considerably beyond the various kinds of differences among the activities themselves. The law of assimilation acts no more independently of the laws of contiguity, frequency, context, intensity, than do these other laws operate independently of it or of each other. Early in the course of establishment of a conditioned reflex the range of assimilated stimulations is very wide; later assimilation becomes more and more restricted.<sup>19</sup> Similar relations have long been known to exist in the case of human reactions in more complicated situations. The asso-

<sup>18</sup> *Conditioned Reflexes* (translated by Anrep, 1927), Lecture VII.

<sup>19</sup> *Conditioned Reflexes*, p. 115.

ciation between the sight of a dog and the speaking of "doggie" may be at first an association between all quadrupeds and the same reaction of speech. That the operation of assimilation is as markedly influenced by factors other than frequency is quite certain. The exact nature of the relations involved is, of course, largely unexplored wilderness. We have enough knowledge on this point, however, to show how unwarranted is the general statement that degree of assimilation is determined simply by similarity. It is determined by a wide variety of factors, or if you choose to mean by *similarity* any kind of relation, then by a wide variety of similarities. And it should be added that the similars in many cases differ about as much from each other as any two things could.

The law of assimilation is concerned with the appearance of functional relationships between activities without any preliminary period during which the development and fixation of that relationship could be observed. To put the matter more simply, this law deals with a certain lack of correspondence between an individual's more obvious psychological history and his present activity. There are other recent attempts to deal with essentially the same facts. The gestaltist, when he sees appropriate behavior in the face of what he judges to be an essentially novel situation, brings forward the principle of insight. We well may ask whether this is not simply a new and rather apt title for a phenomenon also covered by assimilation. The answer is, of course, that while the principle of insight covers essentially the same facts as assimilation, it has important implications that are quite foreign to the latter notion. The principle of assimilation is concerned with a lack of obvious correspondence between past history and present activity, but by virtue of its being, after all, a law of association, it implies that the functional relationship between past and present is, even in such

a case, of the first importance. The principle of insight, on the other hand, seems to emphasize the claim that lack of correspondence between individual history and present behavior illustrates mainly the limits of a genetic point of view.<sup>20</sup>

The facts covered by the law of assimilation are also touched upon by Spearman's principles of education.<sup>21</sup> In his rôle as investigator Spearman has paid a great deal of attention to reactivity to relatively novel situations. It is, therefore, not strange that he has seen fit to postulate as fundamental the individual's ability to depart in his present activity from the literal past. I do not know whether he would be more or less inclined than is the gestaltist to minimize the importance of psychological history in the understanding of originality. Probably he would hold that the associationist makes entirely too much of past history and that the fact of originality deserves better than to be subordinated to such a doctrine as association, which holds that originality is more apparent than real.

A defense of the law of assimilation with its subordination of originality to association has been so vigorously stated by Thorndike that it is worth while quoting him at this point:

" . . . There is no arbitrary *hocus pocus* whereby man's nature acts in an unpredictable spasm when he is confronted with a new situation. His habits do not then retire to some convenient distance while some new and mysterious entities direct his behavior. . . .

<sup>20</sup> My interpretation here and elsewhere in this essay of the conception of insight is based as much upon what I have heard members of the gestalt school say as upon what has been written on the subject. Writers of that school have, as is well known, abstained from any exhaustive statement of their own intentions in respect to this concept.

Since this footnote was first written Köhler's *Gestalt Psychology* has appeared. Chapter X of that work is devoted to insight. After reading it I feel that my statements about insight can stand.

<sup>21</sup> *The Nature of Intelligence and the Principles of Cognition* (1927).

“Were the situation so utterly new as to be in no respect like anything responded to before, and also so foreign to man’s equipment as neither to arouse an original tendency to response nor to be like anything else that could do so, response by analogy would fail. For all response would fail. Man’s nature would simply be forever blind and deaf to the situation in question. With such novel experiences as concern human learning, however, man’s responses follow the law that a new situation, *abcdefghij*, is responded to as *abcdelmnop* (or *abcdeqrstu*, or *fghiabyd*, or the like) which has an original or learned response fitted to it, would be.”<sup>22</sup>

Two points are clear. First, there is certainly a variable degree of novelty in all of our psychological activities. Second, the influences of formerly established associations are never left behind. The inclusion, within the basic principles of association of the law of assimilation, takes care of both of these facts. Some of the critics of the theory of association (this is surely true of the gestaltists) say or imply that the greater the degree of novelty in a given activity, the less applicable is the theory of association to the explanation of that activity. But this judgment is quite unwarranted. It is true that the more novel an activity, the more difficulty there will be in tracing its history. There is a certain tautology in saying so, however, because genetic obscurity is, after all, the test of novelty. And, surely, the observation that the application of a theory to certain facts is more difficult than its application to certain other facts does not constitute an argument against the generality of the theory.

As a matter of fact, such novel activities as seem at first glance highly resistant to explanation in ontogenetic, associational terms are constantly being brought into line with the theory of association. This is nowhere more true than in the domain of psychological disorders. The far-fetched symptoms of the hysteric and the psychasthenic are often

<sup>22</sup> *Educational Psychology*, 1913, II, 28-29.



as original as anything one could wish for. Yet it is now widely accepted that they all hold stories of subtle associative connections and their attendant assimilations.

It has seemed to me that the gestaltist, in setting up insight as a fundamental principle of his psychology, is simply confessing lack of patience. For the associationist, the fact of novelty in an act is a challenge. It is an invitation to investigation. To the gestaltist novelty presents no problem because he postulates as a basic characteristic of psychological activity its tendency to be novel. Of course the peculiar thing about it is that this same gestaltist struts as though he had solved the problem that is worrying the associationist, when all he has really done is to refuse to consider the problem soluble. The gestaltist can give himself better justification by saying that he is not interested in ontogenesis than he can by saying that he has made headway where the associationist has been repulsed. The associationist himself postulates a tendency toward novelty when he states the principle of assimilation, but he leaves open for investigation the problem as to why the particular manifested connection is so unlike the earlier apparent connection or connections in which it has its origin.

It is one of the merits of the theory of association that all of its postulations simply affirm relationships requiring investigation. The law of contiguity sets dozens of empirical problems, and so do the other laws, including that of assimilation. If all of these problems had already been solved, as critics of the theory think they should have been, the theory would no longer be of much account.

### An Extension of the Law of Assimilation

In usual statements of the law of assimilation, it is said that the spread involved is in regard to instigator, or stimu-

lus. That is to say, having established such a connection that *A* will instigate *B*, activities other than *A* will vary in their capacity to arouse *B*. But there is a complementary and equally important fact that should be stated at the same time:

*Whenever an associative connection is so established that an activity, A, becomes capable of instigating an activity, B, that same A will vary in its capacity for instigating certain activities other than B.*

I am introduced to a stranger, and, the next time I meet him, I say "good morning." Although this connection was not manifested in the situation in which associative establishment took place, it was potentially there.

Practically everything said in the preceding section in regard to assimilation of instigators is as applicable to the assimilation of instigated processes.

In summary of both phases of the law of assimilation, it may be said that the law contradicts the claim that associative connections are between absolutely specific items. Both instigating and instigated activities are always more general than any single observation of them is capable of revealing. Nevertheless, the law of assimilation furnishes a sound basis for analysis. The procedure is to take some one manifestation of an associative connection, or some group of such manifestations, and to note the principles governing departures from that reference point. Only chronic absolutists will object to analysis dependent upon such a relatively arbitrary point of departure. The feelings of such absolutists can be saved by the hypothesis that back of these connections between general terms are connections between absolutely specific nerve elements, but I believe that it is undesirable to make the theory of association in any way dependent upon such an hypothesis.

Critics of the theory of association have insisted that the theory is embarrassed by the recurrent novelty of psychological activity. But departure from literal stereotypy is only what the law of assimilation would treat as a normal associative phenomenon. Originality is something which any current theory of association must, itself, predict.

## Chapter V

### THE "LAWS" OF ASSOCIATION (Continued): FREQUENCY, INTENSITY, DURATION, CONTEXT, ACQUAINTANCE, COMPOSITION, INDI- VIDUAL DIFFERENCES

#### The Law of Frequency

PERHAPS the most popular of all laws of association is that of frequency. "Practice makes perfect" is a fundamental principle in Everyman's psychology. To many psychologists the import of the law is too obvious to require explicit statement. Among those who do attempt a precise formulation the following would receive fairly general acceptance:

*“. . . other things being equal, the more frequently a connection has been exercised the stronger the connection.”*<sup>1</sup>

The other things that have to be equal are ordinarily assumed to be such factors as are included in the other laws of association. In the particular statement which we have here taken as an illustration, more than that is probably meant. Gates notes, in passing, that it is only up to a certain physiological limit that increasing frequency is productive of increasing strength of association.

Empirical investigations of the law of frequency have for the greater part been contingent upon the notion of the curve

<sup>1</sup> A. I. Gates, *Elementary Psychology* (2nd ed., 1928), p. 282.

of learning. The form of this curve has been investigated for a great variety of associative connections and concerning it there has developed an extensive literature of controversy as well as of objective results. From the theoretical standpoint it is unfortunate that practically all of these curves of learning represent the influence of frequency upon highly complex sets of associative connections rather than upon connections between relatively isolable acts. However, since such a large part of the existing evidence as to the nature of the influence of frequency is contained in the literature of the learning curve, the outstanding facts and issues of that literature may well be set forth briefly at this point.

The matter may be appropriately entered by means of the question: Is there a single law of frequency or many such laws? That is to say, has the curve of learning a single typical form or a number of different typical forms?

A survey of the empirical results reveals such relevant facts as the following: (1) The majority of learning curves exhibit a deceleration as they approach the upper limiting number of trials. This characteristic is not only present in the majority of learning curves, but it is in almost all of these cases the outstanding characteristic. (2) A substantial number of learning curves show a positive acceleration in their early stages. So far as empirical data are concerned the phenomenon of early acceleration is not nearly so frequent as that of late deceleration. (3) Definite periods of zero progress or very slow progress falling between periods of more rapid progress are occasionally found under circumstances that give them the complexion of something more than statistical accidents. But whereas these plateaus, because they happened to turn up in the results of the pioneer investigations of Bryan and Harter, were once assumed to occur more often than not, they are now known to be relatively

infrequent. They probably never occur except in curves for extremely complex sets of associations, and they surely do not always occur even in such cases.

Most writers would agree that the typical curve of learning is a curve of general deceleration. Some would claim that the initial acceleration is also prevalent enough in the empirical results to justify the conclusion that the typical learning curve has this acceleration. The fact that the initial acceleration is absent from so many empirical curves is explained most often in terms of the fact that the observations in question did not begin until the particular learning process was well under way.<sup>2</sup> One argument for the importance and generality of the early acceleration is that it appears in the curve resulting from an experiment on a conditioned reflex where the connection was relatively simple and well controlled.<sup>3</sup> Another argument calls attention to the fact that, with early accelerations, the learning curve has the general form of the normal probability function. What may be an almost equally strong argument against the generality of initial acceleration is the fact that it was not obtained in certain investigations of the curve of memorization except possibly where the learning process was more complex, and not simpler, than usual.<sup>4</sup> We are thus in a position where we can attach initial acceleration to *any* typical curve of learning only by adopting an hypothesis concerning uninvestigated regions of the curve. Since the actual zero point for any learning process is always obscure in the life history of the individual, it is quite impossible ever to demonstrate a learning process totally lacking in

<sup>2</sup> J. C. Chapman, "The Learning Curve in Typewriting," *Journal of Applied Psychology*, 1919, 3, 252-268.

<sup>3</sup> N. Kleitman and G. Crisler, "A Quantitative Study of a Salivary Conditioned Reflex," *American Journal of Physiology*, 1927, 79, 571-614.

<sup>4</sup> E. S. Robinson and M. A. Brown, "Effect of Serial Position upon Memorization," *American Journal of Psychology*, 1926, 37, 538-552.

acceleration somewhere beyond zero achievement. There is probably no known learning process the zero point of which can be observed and, as long as any region of the curve beyond the zero point has not been plotted, there is the ever-present possibility that the acceleration might occur in that region. On the other hand, it is probably equally impossible to demonstrate empirically that every learning curve would show acceleration in its lower reaches if it were sufficiently extended in that direction. The conclusion to be reached from these considerations is clear. So far as we are interested in generalizing empirical results, we are justified in ascribing to the most typical curve of learning general deceleration in its upper reaches and that is about all. If we are concerned with what the curve would be, were it projected down to some absolute zero, we may surmise on the basis of a combination of empirical evidence and conjecture, that it would exhibit early acceleration. But the generalization of curves as they have been secured from actual observation ought to be kept quite distinct from hypothetical extensions of that generalization.

Although we are not justified, on empirical grounds, in ascribing early acceleration to the most typical curve of learning, there is some doubt about the wisdom of claiming that the most typical curve is lacking in such acceleration. While one could make a survey of all of the learning curves exhibited in the literature of experimental psychology and determine the relative number possessing this characteristic, the count would be of questionable significance. My estimation is that early acceleration would fail to appear somewhat more often than it would appear, but another decade of experimentation might easily reverse that situation. The important fact is that the curve of learning occurs frequently with and frequently without early acceleration.

In the case of the plateau we are undoubtedly justified in

saying that, most typically, this feature of the learning curve is absent. This does not mean, however, that the plateau is to be ignored as a pure eccentricity. It is a feature of the curve which, under reasonably determinable conditions, can be present, but as a rule is not.

There are, of course, psychologists who are far from content with such rough summarizations of the curve of learning as those with which we have here been dealing. Beginning with Thurstone,<sup>5</sup> a number of investigators have sought to give a definite mathematical statement of the curve of learning. That equations can be found to express individual curves of learning, nobody would deny. It is another question, though, as to whether the data of *all* curves of learning can, in their original form or in some secondary form, be subsumed under a single mathematical law. The burden of proof still rests upon the mathematical enthusiasts. Not many of us would be disposed to take seriously the idea of Kleitman and Crisler<sup>6</sup> (suggested by a related point of Robertson's in respect to the curve of forgetting) that the curve of learning so closely approximates in mathematical nature the law of autocatalytic action, that the learning process may be assumed to be at bottom an autocatalytic phenomenon.

It would be foolhardy to deny the possible application of mathematical methods at any given point in the realm of

<sup>5</sup> L. L. Thurstone, "The Learning Curve Equation," *Psychological Monographs*, 1919, 26, No. 114; M. F. Meyer and F. O. Eppright, "The Equation of the Learning Function," *American Journal of Psychology*, 1923, 34, 203-222; R. V. Blair, "Thurstone's Method of Study of the Learning Curve," *Psychological Review*, 1918, 25, 81-83; P. L. Jette, "A Study of the Learning Curves for Two Systems of Shorthand," *Journal of Experimental Psychology*, 1928, 11, 145-160; M. C. Barlow, "A Learning Curve Equation as Fitted to Learning Records," *Psychological Review*, 1928, 35, 142-160; L. L. Thurstone, "The Learning Function," *Journal of General Psychology*, 1930, 3, 469-491.

<sup>6</sup> *Op. cit.*, p. 603 ff.



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scientific fact, and it would be ridiculous to contend that a way will not ultimately be found for bringing every individual learning process under a single mathematical law. There are certain outstanding difficulties, however, which intellectual sobriety should not forget. The curve of learning gives the relationship between strength of associative connection and frequency of repetition of the operation of that connection. But frequency of operation is only one of the variables with which strength of association is correlated. The factors of time-interval, assimilation, intensity, and set are also important determiners of associative strength. Since this is the case, a single mathematical law of learning is possible only if all of these factors other than frequency can be kept constant, or only if they can be proved not to influence the form of the learning curve. We know they can never be reduced to zero, so we have a right to ask what are the specific conditions of these other factors when the general law of learning is manifested or what is the proof that these factors can be neglected. Another barrier in the way of a single mathematical law of learning is the commonly acknowledged fact that the merely arithmetical difference involved in various common methods of measuring associative strength are such as to preclude the possibility of one law representing the same learning process as scored by divergent, though equally legitimate, methods. For example, where achievement is scored in terms of amount of performance per unit of time, we cannot get the same curve as we do where achievement is scored in terms of time per unit of performance. This difficulty might conceivably be overcome simply by arbitrary agreement and so it is not quite as serious as the one first considered.

While studies of the curve of learning have furnished us with the greater portion of our empirical data on the nature of the law of frequency, there are other psychological in-

vestigations that must be taken into account before we can secure a really broad view of the influence of this factor. The learning curve is plotted from the results of experiments so arranged as to produce generally increasing associative strength with increasing frequency of operation of the connections in question. If the conditions of an experiment are such that increasing frequency of operation results in decreasing associative strength, then the curve representing this fact is called a curve of work rather than a curve of learning. The fact that the increasing associative strength emphasized by one class of experiments has been given the special title, *learning*, and the decreasing associative strength emphasized by the other class of experiments has been given the special title, *fatigue*, or *work decrement*, has led us to forget that, in both cases, we are dealing with the influence of frequency upon the same variable. A recognition of this state of affairs should make us critical of the easy statement that: "*other things being equal, the more frequently a connection has been exercised the stronger the connection.*" If we are willing to keep in mind experiments on "fatigue" at the time we are considering experiments on "learning," we must conclude that frequency is associated with decreasing strength of association about as definitely as it is with increasing strength of the same kind of connection. Surely, the decrement in the curve of work represents something more than a mere exception to the law of frequency. Statements of the law of frequency, like that one just given, ordinarily include the proposition that frequency causes increasing associative strength only to the extent that other things are equal. But it begins to look as though those other things are much more than incidental and that a fairer phrasing of the law would be to the effect that increasing frequency produces increasing associative strength when conditions are not ripe for this factor to have an opposite or a neutral effect.

One other class of experiments needs to be introduced to clarify the present situation. For years a popular problem among experimentalists has been that which has gone under some such title as, "Optimal Distribution of Practice."<sup>7</sup> Unlike the learning curve and the work curve investigations, which simply study the alteration in associative strength as more and more performances are added, the typical experiment in this group varies the time intervals between these performances and studies the effects of such variation upon the influence upon association of a given number of performances. The concern is with frequency, but it is with frequency per unit of time rather than with absolute and accumulative frequency. It is apparent that if any simple generalization could be made in regard to "optimal distribution of practice" it would be of great practical value. This consideration has, unfortunately, been so prominent in the minds of most investigators in this field that there has been little attention paid to the actual complexity of the problem.<sup>8</sup> Individual investigators have been inclined to generalize upon the wide subject of distribution of practice on the basis of their own very limited results. As it has turned out, there have been more experiments showing greater improvement with *less* frequency per unit of time than experiments yielding the reverse result. Nevertheless, it is evident that no simple generalization to the effect that less relative frequency is more conducive to associative strength than more relative frequency is warranted. We may be sure that the opti-

<sup>7</sup> C. J. Warden, "The Distribution of Practice in Animal Learning," *Comparative Psychology Monographs*, 1, 1923, contains a good historical review.

<sup>8</sup> L. A. Pechstein ("Massed vs. Distributed Effort in Learning," *Journal of Educational Psychology*, 1921, 12, 92-97) was one of the first to recognize the almost certain probability that the effectiveness of a given distribution depends upon factors other than distribution. He himself demonstrated the close relation of the distribution question to the method of learning—part learning vs. whole learning.

mal relative frequency for almost any given connection will usually lie between lesser and greater relative frequency values. The principles of refractoriness and fatigue are limiting factors on one side and the principles of forgetting and warming-up are limiting factors on the other side.

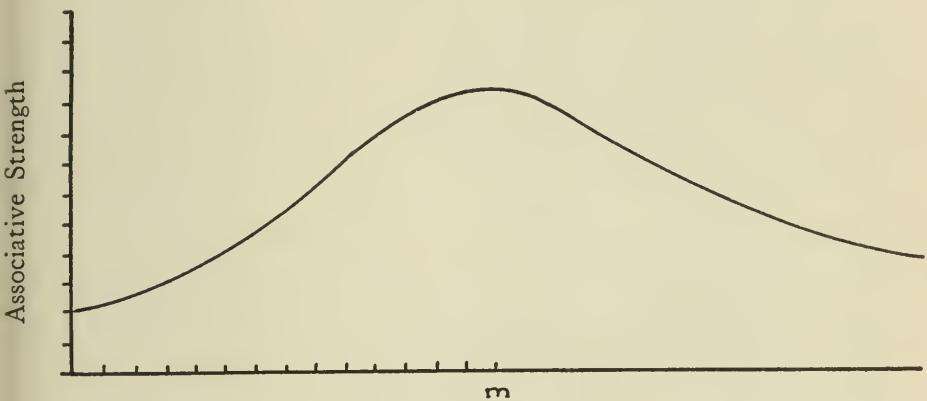
If we are to have a single general law of frequency, it is necessary to deal with the relative frequency represented by the experiments on distribution of practice rather than with the absolute frequency of the learning curve and the work curve investigations. In the case of absolute frequency there is no single general relationship between that factor and associative strength. From zero frequency to maximum frequency there may be an increase or a decrease in associative strength. But in the case of relative frequency, there is presumably a medium frequency somewhere between maximum and minimum that will produce the greatest associative strength. This is represented by the accompanying figure. ~~With~~ very low frequency we have the deleterious effects of disuse or forgetting, ~~With~~ very high frequency we have the deleterious effects of "fatigue." With certain intermediate frequencies we have positive practice effects. The absolute character of this law will almost surely differ for every associative connection or set of such connections, but typically we may expect a functional relationship marked by a large inversion of the first order.<sup>9</sup>

Such a law as this obviously has limits. For example, it does not hold where the period within which the repetitions occur is so short that only a very few repetitions are permitted.

In light of such facts as have here been under discussion, it does seem strange that psychologists should get ex-

<sup>9</sup>The zero frequency is accompanied by a small positive associative strength because of the fact that *absolutely new* connections are probably never formed.

cited upon discovering that greater frequency does not always make for greater associative strength. Is it not strange that there could be such easy forgetting of the very obvious fact that work curves and learning curves are plotted on the same coordinates. The layman, unless he thinks about the matter for two moments, will be astonished to hear that practice makes imperfect as well as perfect. But



Frequency of Presentation per Constant Total Period of Time—The Actual Values are Hypothetical.  $m$  = Some Intermediate Frequency

FIG. 2

The General Law of Relative Frequency

we should hardly expect astonishment on the part of the psychologist.<sup>10</sup>

The Law of Intensity

It is widely recognized that degree of association is a function of the intensity of one or both of the activities that

<sup>10</sup> On page 129 more will be said in explanation of the fact that increasing frequency is often accompanied by decreasing or stationary associative strength.

are becoming associated. Thomas Brown gave formal expression to the principle in the following terms: "The parts of a train appear to be more closely and firmly associated as the original feelings have been lively."<sup>11</sup> It is probably because of the apparent familiarity of the principle and of the facts which it is alleged to represent, that the law of intensity has been given little critical attention.

Perhaps the first point about this law to deserve inquiry is the term *intensity*, which has, on several accounts, an unsavory reputation in psychology. A return to Brown's "liveliness" would hardly help matters, because that term is only a popular one and its meaning may be expected to be even looser than that of *intensity*. We have been taught the great importance of distinguishing between sensory intensity and stimulus intensity. Consideration of the earlier discussions of the stimulus and its place in associative determinations (p. 29) would show that the law of intensity is probably not a law of stimulus intensity. The earlier discussion of sensory process (p. 42) would show that sensory intensity might be directly involved in the determination of association.

The nature of the law of intensity must depend, among other things, upon whether sensory process and simple feelings are factors of a fundamentally different order. It is usually assumed that simple feelings vary in intensity, but, if these feelings are radically different from sensory processes, then we should hardly assume that intensity is an identical dimension in the two cases. And, if intensity of feeling and intensity of sense process are different, then it is necessary to know whether associative strength is a function of one or of both, and, if of both, whether the type of relationship is the same in the two cases.

<sup>11</sup> *Lectures on the Philosophy of the Human Mind* (1820), II, 44, Lecture 37. Quoted by Warren, *History of the Association Psychology*, p. 73.

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Since it is still a matter of unsettled controversy whether sense processes and feelings belong in the same or separate categories, the prospect of effecting an early settlement of the questions immediately before us is not bright. Most writers, if they give any evidence of their attitude in the matter, seem to imply that associative strength is a function of both kinds of intensity. As a rule, the main preoccupation is with affective intensity. Here prevalent opinion would assert a positive relationship between associative strength and increasing intensity of pleasantness and a negative relationship between associative strength and increasing intensity of unpleasantness. Unfortunately, where we can be at all sure of exactly what is meant by intensity, it is not at all clear how closely this relationship does hold.<sup>12</sup>

While the exact meaning and status of the variable, affective intensity, is far from definite, there is no genuine scientific gain to be secured by shifting our attention to the rough-and-ready notions of "satisfying" and "annoying" states of affairs,<sup>13</sup> which, in the practical and impatient atmosphere of recent American psychology have found no little favor. And as far as clarity of conception is concerned, the surreptitious hop from the category of affectivity to the evaluation of "survival values" is but another manner of getting from the frying pan into the fire. Survival value may properly be a concern of psychology, but it is a conception which, in the first place, does not correspond directly with that of affectivity and, in the second place, is quite as resistant to precise definition.

<sup>12</sup> E. C. Tolman, *Psychological Monographs* (1917), 25, No. 107, is one of the few experimentalists who has secured direct evidence on the relationship of affective tone to associative fixation. He found that lists of "pleasant" words were more quickly learned. Most experimental studies have dealt largely with the influence of affective tone upon retention.

<sup>13</sup> E. L. Thorndike, *Educational Psychology* (1913), I, 172-173, and II, 4.

The Law of Intensity reveals still more complexities when we come to the emotional activities. It is a general custom to assume that emotions vary in intensity. But is *this* intensity a dimension of the "affective tone" which is alleged to be so prominent in emotion or does it pertain to the sensory or muscular or glandular components? If intensity, in the case of emotion, is admitted to mean, as it surely does in practice, now one and now another and now a combination of these modes of variation, what is the nature of the relationship between intensity of emotion and strength of association? That there is some relationship is, again, a matter of general belief. That this relationship is a fairly simple one is a belief of almost equal popularity. While we should hardly care to deny the relationship on the basis of our present critical considerations, we should certainly distrust the idea that the application of the law of intensity to emotional activities is a simple proposition.

When Brown used the term *liveliness* he probably had in mind not only such intensities as those we have been discussing, but also a variable which is designated by the term *clearness* or *attensity*.<sup>14</sup> That associative strength is a function of this variable is hardly a matter of debate. Yet we are far from having definite expressions of opinion, let alone facts, concerning the relationship between this kind of "liveliness" and those other intensities that we have been enumerating. If the suggested distinction between attensity and cognitive clearness becomes generally accepted, the complication that we have here added to the law of intensity will probably be doubled, in that cognitive clearness, if it is a legitimate variable, is surely a determiner of associative strength.

Since muscular movements and glandular secretions have

<sup>14</sup> E. B. T., "The Term 'Attensity,'" *American Journal of Psychology*, 1924, 35, 156.



become, in fact,—if not always in philosophical theory—proper and directly accessible data of psychology, the conception of intensity meets other complications. Aside from resulting kinæsthesia and its intensity, the intensity of a muscular movement is certainly a direct factor in determining the degree to which that movement becomes associated with any given associative instigator. An analogous relationship very probably holds for glandular secretion.

The term *intensity* thus seems applicable to numerous variables that are related to the fixation of associative connections. Since the heterogeneous character of this conception has generally remained implicit, we well may ask whether this has been due to the fact that there is a large factor of similarity running through the associative consequences of all of these forms of intensity variation. The more lively, the more closely associated, is the simple rule suggested by Brown and uncritically adopted by many writers since. We have only to look to affective intensity, however, to find another assumption. The more intense the unpleasantness, the weaker the associative connection, is a common theory. While experimental evidence on the question is usually indecisive, or at least difficult of interpretation,<sup>15</sup> and while the real import of the clinical matter on pathological association is still more questionable, there is too strong a suggestion of an occasional inversion of the intensity-association relation to warrant our acceptance of the single, one-way relationship so often predicated.

One thing of which we may be sure is that variations in several kinds of intensity have a pronounced influence upon the readiness with which associative connections are formed. We are able to observe instances where, within a certain range, associative strength is greater with greater intensity. Under what circumstances and how often there may be

<sup>15</sup> See recent articles on Memory in the *Psychological Bulletin*.

inversions of that relationship are matters about which we know very little. The obvious need is for experimentation. Theoretical analysis, such as the present, has as its function the pointing out of several probably significant variables where only one has commonly been acknowledged. But the manner in which those variables manifest their significance must wait upon the observation of their functioning under controlled conditions. One should hardly expect that complete knowledge of all the facts lying within the region marked off by the law of intensity will be the prompt reward of mere experimental diligence. The complexities are exceedingly great. We have no convenient units in terms of which to measure affective intensity, for example, even if we could bring the variable (or variables) under control. And even if we were able to determine the separate influences of the various types of intensity, we should have before us the additional task of determining how these variables affect association when they operate, as they always do, in combination. That the final law of intensity combination will turn out to be a simple summative one is hardly to be expected.

There is one other complication in the law of intensity which is especially important because it has analogues in connection with the operation of other factors of association. Here is the question: Does the variation in intensity (or any other factor) that determines to a degree the strength of association occur in the activity which is to become the instigator, or in the activity which is to become instigatable by a new instigator, or in both? As nearly as I can tell there has been a tacit assumption that change in intensity at any point in a total, complex situation has an effect upon all of the associative connections involved, but I am not at all sure that anyone would wish to defend that assumption if he were faced with it in this way. On the other hand, there is

slight basis for denying the possibility that the assumption is valid, or may under certain circumstances, be valid. Again the need for experimentation is clear. Although there are many detectable variations of intensity that cannot be measured, there is no reason why those variables that can be measured should not be brought under observation. And, in doing so, it should be possible to distinguish between the effects of intensity variations in both the instigating and instigatable activity.

### The Law of Duration

First in order among the nine secondary factors of association enumerated by Thomas Brown stands *duration*. "The longer we dwell on objects, the more fully do we rely on our future remembrance of them."<sup>16</sup> Most of the comments made concerning the law of intensity would be applicable also to the law of duration. In a certain rough way we know that the duration of an activity may influence the degree to which it enters into a given associative connection. And we know that, within limits, increasing duration brings increasing strength of association. It seems quite likely that there is a point beyond which increasing duration will bring about a reduction in associative strength. If such is actually the case, Brown's formulation of this law will have to be altered. The important point would seem to be that the condition of duration which will result in the greatest associative strength lies somewhere between extremely short and extremely long durations.

Experimentation has dealt with the influence of duration. Syllables have been given varying exposure periods in the hope of discovering the optimal distribution of a given total

<sup>16</sup> *Lectures on the Philosophy of the Human Mind* (1820), II, p. 4, Lecture 37. Quoted by Warren.

study time. In the main, the best duration in this sense has been surprisingly short. Warden, in his study of the distribution of practice in animal learning,<sup>17</sup> obtained most efficient learning with the shortest practice sessions. Obviously there is a probable limit to this where a further reduction in duration of a practice session would, by the nature of the case, force a fractionation of one of the activities to be associated. It seems, then, that the relative effects as well as the absolute effects of a given occurrence of associable activities are most favorable when their occurrence has some medium duration.

It should be noted that the influence of duration is not always easy to disentangle from that of frequency. In the experiments above quoted, the conditions of greater duration have been the conditions of smaller frequency, and it is often quite impossible to say which of these dual characteristics is the really effective one. For this reason it would seem desirable to undertake the experimental comparison of single occurrences of associative connections varying in absolute length.

As in the case of intensity, duration should be studied as a variable characteristic of both of the separate terms—instigating and instigated activity.

### The Law of Context

Activities that are to become associated never occur in isolation. The critics of the theory of association make much of this fact. They make little more of it, however, than do wise advocates of the theory. It is convenient to abstract pairs of associated terms, but that is no reason why one should assume that the process of association is not influenced by the context in which the associated activities occur.

<sup>17</sup> "The Distribution of Practice in Animal Learning," *Comparative Psychology Monographs*, 1, 1923.

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In formulating the laws of association the frequent throwing together of principles of fixation, principles of retention, and principles of reinstatement has brought about a certain neglect of the influence of context upon fixation. The familiar statements of the law of congruity or the law of emotional congruity have most frequently dealt with the influence of total situation upon associative reinstatement and neglected the influence of total situation upon fixation. There has, however, been no implication that the process of fixation has been unaffected by context. There has simply been a lack of sufficient analysis.

If we can detect any definite opinion about the effects of context on fixation other than the opinion that they are important, this opinion would seem to be that associative connections are most readily formed in a "congruous" context. But mere casual observation has given no very precise results on the point. Recently Pan has carried out an interesting series of experiments which give a hopeful entrance into this whole complicated problem.<sup>18</sup> He found that in associating pairs of words the process was facilitated by the presence, during learning, of words logically related to the word to be instigated or by words related both to it and to the instigator. But a context logically related to the instigator and not closely related to the instigated term acted to retard fixation. More recently Whitely<sup>19</sup> has sought to determine the influence on memorization of logical material of the arousal, prior to study, of what he aptly calls "congruous apperceptive systems." In other words, the attempt was to discover whether a person who starts to think on a general

<sup>18</sup> "The Influence of Context upon Learning and Recall," *Journal of Experimental Psychology*, 1926, 9, 468-491. See also G. L. Freeman, "The Rôle of Context in Associative Formation," *American Journal of Psychology*, 1930, 42, 173-212.

<sup>19</sup> "The Dependence of Learning and Recall upon Prior Intellectual Activities," *Journal of Experimental Psychology*, 1927, 10, 489-508.

subject such as the American Civil War, will become unusually ready to form associations pertaining to that general topic. Whitely's results fail to show the effect for which he was looking, though this, by no means, eliminates the general possibility. It simply means that Whitely was unable to demonstrate a principle which still seems to be very probably a valid one under certain, as yet undetermined, circumstances.

As Pan, himself, points out, it is possible to interpret his finding somewhat more definitely than merely by indicating a logical congruity between an instigated word and what proves to be a favorable context for its instigator. This fact of logical congruity implies an already existent associative connection between context and instigated word. Therefore, if one keeps in mind the total instigating activity, it can be seen that, under the circumstances that we are now discussing, the association is partially established before formal rehearsal is undertaken.

If the facility with which associations are fixed under favorable conditions of context is reducible to the more elementary fact that there is a considerable degree of fixation to begin with, it may be relevant to ask whether the law of context is, after all, fundamental. The principles accounting for the established association by virtue of which a given context gains its potency may be simply those principles of contiguity, frequency, intensity, and duration which have already been dealt with. Even if this be true, however, there is a real function for a law of context. Until this law is stated there is no clear acknowledgment that the associative possibilities of any given activity may be dependent upon accompanying activities. In Pan's experiment something more took place than the strengthening of the connection between the instigated word and the context words. A largely new connection was facilitated. The fact that the

operation of an associative connection is a favorable (or unfavorable) condition for the formation of other connections is not deducible from the general principle of association. It is a special instance which requires its own statement.

There is a peculiarly close relationship between the law of context and the law of assimilation. Analysis in terms of the first of these asks what varying associative powers can be taken on by a given activity that occurs in varying context. Analysis in terms of the law of assimilation asks what identical associative powers can be taken on by a class of activities varying from each other in some specified manner. The emphasis in one case is upon a central term surrounded by other terms capable of influencing its associative possibilities. The emphasis in the second case is upon classes of activities and the variations in associative powers to be found within the classes. It is clear that there may be overlapping of these laws. The fact that the sight of a certain man, as instigator, arouses me now to say "Good morning" and now to say "Good evening" may be looked upon in terms of the assimilation by an instigator of a number of instigatable activities. Or the same fact may be interpreted in terms of the varying contexts of the same instigator and the varying associative powers of those contexts. Analysis in terms of influence of context tends to reduce all associative connections between highly complex terms to a great multiplicity of connections between simpler terms, while analysis in terms of assimilation tends to point out modes of variation in associated terms in relation to the associative possibilities of those terms. One analysis works more in structure-like units; the other more in modes of variability. If it were possible to choose between the two outlooks on logical or methodological grounds the situation would be simplified by our doing so. I believe, however, that no such choice can be made. Scientific analysis in different fields seems to swing

back and forth between a preference for relatively stable units and more clearly conceptual guiding lines, such as definable dimensions of variability. As long as this pendularity continues we are safer in assuming that both points of view are merely points of view and that therefore there is no reason for not exploiting both. Child's conception of dimensions of metabolic gradation does not do away with all usefulness of such stable notions as that of cell. Neither does the law of assimilation, because it may be made to embrace the law of context (as it can be made to be embraced by that law) eliminate the usefulness of the law of context.

But aside from these more or less philosophical considerations, the plain fact is that, though an extensive overlapping is apparent in the phenomena designated by the laws of assimilation and context, there is no ready formula for making detailed translations from one law into the other. We are therefore on firmer ground to use both. Only the persistent absolutists are likely to be made uncomfortable, and their case is too generally hopeless to justify special solicitude in this connection.

The application of the conception of context to a complex situation is not ordinarily balked by any confusion between the context and what we might call the central item or items. I say "Good morning!" to my friend seen in one context and "Good evening!" to the same friend seen in a different context. There is no great difficulty here in deciding what is central item and what is context. The fact that there is an ambiguous region somewhere between context and central item is of no great consequence. Among the several factors here involved in instigation, the more variable ones are relegated to the category of context and the less variable to that of central item. The distinction between context and central item may also be based upon the different levels of attention, the notion of context taking in that which



attentively is in the background. A third basis of distinction corresponds to differences in the specificity and generality of the items involved in a given associative connection. An association is established between white and black under a general attitude that means opposite. One could say that an association is established between the attitude, opposite, and the word, black, of such an order that the speaking of the word, black, will be instigated by the attitude, opposite, whenever that attitude occurs in the proper context, that is to say, whenever it is accompanied by hearing or seeing the word, white. I say this is logically possible, but it would not fit into our usual methods of thought because they imply that the more specific factors are to be considered central.

With these several bases for distinguishing between context and central item, confusion is ordinarily absent only because the basis for making the distinction in any given case is fairly clear. At least the confusion is no greater than the general intricacy of the facts involved. In further study of the law of context, however, it would be well for us to lay down with some degree of definiteness exactly what is to be the basis for distinguishing, in a given case, between context and central item. It is not at all a far-fetched possibility that sufficient knowledge will reveal a number of relatively distinct laws within the area now roughly marked off by the law of context.

### Other Laws of Associative Fixation

The laws of contiguity, of assimilation, of frequency, of intensity, of duration, of context, have turned up so persistently in psychological writings that we may be confident of the importance of the factors that they indicate. Nevertheless, the enumeration of these generally accepted principles is not fully satisfying to the experimentalist. He is

acquainted with an elaborate body of empirical literature on association, or learning, and the findings therein seem inadequately expressed by these laws, most of which were first formulated without the support of experimental observation. Some important possibilities may occur to him. There may be, in the more recently obtained knowledge of association, grounds for adding to the list of generally accepted fundamental principles of association. This more recent knowledge may also add to our fundamental principles not by entirely new formulations, but rather by giving support to principles previously formulated, yet hitherto not widely accepted. In Thomas Brown's list of nine secondary laws of association are some, such as that regarding individual differences, which have a very modern sound. Another possibility is that such principles as recent knowledge can add to those already discussed are not of the same order of fundamentality.

It is my belief that we are now in a position to add to the list of basic principles of association others that are quite as important as the generally accepted ones. We cannot do so, however, simply by listing all of those factors that have been found to influence associative fixation. While no one would question the difference in efficacy between remote and resident control in the formation of certain sensory-motor habits, one would hesitate to list remoteness of control as a factor of the same order as contiguity, or frequency, or context. Similarly the relative merits of the *whole* method of learning as contrasted with the *part* method do not seem to be based upon some one fundamental principle of associative fixation.

It should, of course, be readily admitted that any attempt to list those additional factors of association that really are fundamental is certain to make errors of both omission and commission. There are reasons, though, why the effort is

worth its risks. The need of pedagogy for an economics of study has led to an undifferentiated listing of all of the factors of practical consequence in the formation of associative connections. Where learning has been treated with less regard for use and more regard for theory, there has been a sort of tacit assumption that the traditional laws of association, or, perhaps, even one or two of them, cover the problem adequately at its most fundamental level.

Perhaps the greatest difficulty in the way of distinguishing between the more and the less fundamental factors influencing associative fixation lies in the meaning of "fundamental." A principle is more fundamental than other principles which can be translated into terms of that first principle, providing, of course, that the translation cannot be made just as readily in the other direction. If, in a particular instance, the translation seems as readily made in one direction as it does in the other, reference must be made to other instances. Furthermore, that one of the principles under consideration which has the widest reference will be judged the most fundamental. Such criteria are often difficult to employ. They are certainly very subtle in the case of the present problem, yet we cannot refuse to seek an impression of them. But in most cases we shall do well not to enter into any elaborate defense of the fundamentality of this factor and that. Precise knowledge of the mode of operation of even such a factor as contiguity is entirely too fragmentary to justify any great concern about the proof of its finality.

### The Law of Acquaintance

~~There is some reason to believe that any given case of associative fixation depends, not only upon existing connections between an individual's activities, but also upon what activities he has engaged in.~~ Let us examine a relevant case.

In one of the classic experiments of Ebbinghaus a list of syllables was learned and then the same syllables were relearned after their order had been altered in one of several ways. In most cases the relearning was done more readily than the original learning.<sup>20</sup> A popular interpretation, and one which Ebbinghaus himself adopted, maintains that in the original learning connections were formed between each syllable and each other syllable. The connections between adjacent syllables were strongest, but they were by no means the only connections formed. If such an explanation best fits the facts, there is no need for assuming our law of acquaintance. There is another explanation, however, that has certain arguments in its favor. When a given associative connection is formed, such as X — Y, there have been three distinguishable occurrences, X, Y, and the connection between them. Now the place-skipping theory of Ebbinghaus assumes that the fact of connection can influence the future associations of an individual, but nothing is said of the possibility that the act of reading or reciting a given syllable may, irrespective of its associative connections, influence the facility with which that act enters into new associations. Yet there is certainly some reason for making the second assumption.

I am not arguing that place-skipping associations do not occur and play a part in determining the facility of new fixations. But I do believe that this factor of acquaintance must also be taken into account. It is often alleged that there is a certain educational value in hearing a foreign language that one is seeking to master even if one has no idea of what the conversation is about. Is the value here to be accounted for entirely in terms of subtle associations upon which future learning can be based? I do not think so. I believe that there is a value in hearing the words, which can be advantageously

<sup>20</sup> *Memory* (1885, tr. 1913 by Ruger), Chapter IX.

abstracted from any associative connections into which they enter. If this proposition cannot now be proved, neither can the contrary one that only the factor of prior connection is really effective. Since this is the case it is wiser to postulate a principle of acquaintance, because only by so doing are we likely to keep the full possibilities in mind.

### The Law of Composition

There is another of the general principles of association which hinges upon the nature of the activities that are associated. Given any activity A, there are differences in the readiness with which that activity can be associatively connected with other activities, quite aside from how familiar those other activities are in themselves or to what degree they have previously become associated with A. In general, the overt speaking of a word or sentence is more easily associated with other activities than is subvocal speaking of the same word or sentence.<sup>21</sup> Although direct experimental access to them has seldom, if ever, been secured, there are undoubtedly other intrinsic characteristics of activities which enter into the determination of their general susceptibility to association.

From what we know of the intricate architecture of the nervous system and from our constant experience with the high variability of psychological events, we are justified in stating a further possibility regarding the influence of intrinsic characteristics of activities upon their associative possibilities. It is altogether likely that the nature of an activity affects not only its general susceptibility to associative connection but also its susceptibility to particular associations. According to our first statement, gross muscular movements,

<sup>21</sup> See M. C. Barlow, "The Rôle of Articulation in Memorizing," *Journal of Experimental Psychology*, 1928, 11, 306-312.

for instance, may on the average be more susceptible to association than other activities, such as imagery, delicate movements, and so on. But it is quite possible that certain image processes have an exceedingly strong tendency to become associated with certain other image processes because of the intrinsic natures of these processes.

The fact that the final empirical test of such propositions as these lies in the future, need not dissuade us from keeping them in mind. Perhaps the chief advantage of holding before us a possibility that has a certain credibility but no adequate means of proof is that we are thereby armed against the unwarranted acceptance of an assumption which denies this unproved proposition. In the present case, denial of what has been stated as the probable condition of affairs implies the assumption that the intrinsic nature of associated activities has *nothing* to do with their susceptibility to associative connection. And who would choose such a guess while he is still able to make the one which we have here proposed. Köhler has recently claimed that a denial of such a law is a basic feature of association theory.<sup>22</sup> But this is simply an attempt to make the conception of association as untenable as possible.

Before this law of composition becomes as approachable as certain of our other basic laws of association, some thought must be given to the variety of intrinsic characteristics of which associative susceptibility may be a function. It has been suggested that, with meaning constant, or practically constant, associative susceptibility varies with the muscular constitution of the act that does the meaning. It seems probable that within certain limits, acts involving the larger, freer use of muscles have the greater associative susceptibility. If this were all, we might subsume the law of composition under the law of intensity, but there is no reason

<sup>22</sup> *Gestalt Psychology* (1929), p. 294.

to believe that intensity variation even with its manifold forms is the only type of variation in the internal characteristics of acts that can affect their readiness to enter into associations. Still, the exact character of those other variables is a complicated problem that has, so far as I know, hardly been dealt with up to the present time.

### The Law of Individual Differences

*The rate at which any given associative connection can be fixated depends, among other things, upon a factor of idiosyncrasy which cannot be adequately explained in terms of the individual's personal history.* Although there may be an occasional psychologist who would reject such a principle even in this general form, his criticism would probably hinge upon some unusual interpretation of the terms involved. The principle is accepted by the majority of competent thinkers.

The fact that individuals differ in their innate capacity to form associations of a certain type is taken by Thorndike to underlie intellect, which is, of course, one of the most important conceptions in modern experimental psychology.

"As a result of the high correlations found between mere associations or connections and the so-called 'higher' processes of abstraction, generalization, organization and control, inference and reasoning, we have advanced the hypothesis that the original basis for altitude, width, and area of intellect is the mere number of possible connections in the neurones whose connections correspond to having and using ideas." <sup>23</sup>

We need not debate the necessity or the desirability of the physiological implications included in this statement. As Thorndike himself says, "The physiological facts in the neu-

<sup>23</sup> *The Measurement of Intelligence* (1926), pp. 473-475.

rones which produce and, in the deeper sense, are intellect, are not known. When they become known, they may or may not be amenable to observation and measurement.”<sup>24</sup> The important point is the explanation of intellect in terms of capacity for forming association of a certain type.

As was shown in an earlier chapter of this essay (p. 36) there are many associations that are not wholly or dominantly ideational. If, as Thorndike and others seem to feel, capacities for ideational association can be grouped, a question arises as to what other groupings of associative capacities are valid. The ability to acquire muscular skills and the ability to form emotional habits are suggested classes, though neither can be given as good statistical justification as intellect.

### Are the Laws of Association Laws?

In the foregoing pages we have been as much concerned with what is not known about the operation of various factors of association as we have with what is known about these factors. We have considered more principles of associative fixation than are usually included under the laws of association, and we have found, furthermore, that the traditions regarding such factors as have long been admitted to positions of importance are in need of considerable modification. For example, the notion that contiguity is a simple qualitative fact constituting a *sine qua non* for association is not acceptable. In its place there must be the more general statement that associative fixation varies with the time-interval between the occurrences of the associated terms. But exactly how that variation occurs or the number of ways in which, under different circumstances, aside from time-interval, it may occur are matters that can be dis-

<sup>24</sup> *Op. cit.*, p. 473.



cussed only in the most general way or only with reference to relatively isolated results like those of Yarborough. Similarly in our examination of other factors of association, we have found multitudes of possibilities opening up and tending to minimize the accuracy of former, over-simplified statements, but we have in most cases been left merely with possibilities and probabilities.

The state of mind into which we have come is a wholesome one in that we have placed strong emphasis upon problems that require investigation. The fact that we have thrown over most of the simple and final facts can be a worry only to him who prefers book-learning to investigation.

There is, nevertheless, an opportunity for honest doubt whether our critical arguments have not proved merely the nonexistence of any scientific laws of association. I am not greatly interested in this question, but some answer ought to be made to it. I have continued to use the term *law* in connection with the various factors of association merely because it has been the custom to do so and because I have felt that a departure from this custom ought not to be made without justifying it at the time. I also have some doubt as to how definitely the operation of a factor must be known before that operation can be said to express, or be expressed by, a scientific law. If the factor of contiguity were really a simple, qualitative, *sine qua non* of all association, would there be a "law" of association in a sense in which there is not when we recognize that we are dealing with a relationship between two continuous variables—time-interval and degree of association? If the answer varies from *yes* to *no* according to the definiteness of our knowledge of that latter relationship, what is the degree of definiteness required before the dignity of "law" is approached? Clearly we are here in a realm of arbitrary decision and I do not

think that it makes a great difference just what the decision is.

It is my firm conviction that the facility of associative fixation is a function of all of the factors enumerated. Probably several specific relationships are involved for each named factor and almost certainly there are other factors which have not been included in this list. But, if the assumption that these factors are important determiners of association be correct, then there are "laws" of these factors whether our knowledge of them is definite or not. If this point hinges upon a realistic philosophy, it is only the realism by which science lives her everyday life, whatever scientists may say when they make up stories about her.

## Chapter VI

### ASSOCIATION THEORY TO-DAY

THE preceding treatment of the theory of association has been based upon the assumption that an essentially valid and useful scientific conception can emerge as a product of intellectual evolution. The modern form of such a theory is naturally open to critical attacks that are not really relevant to this modern form, but which were relevant to features possessed by the theory at some time during its past history. In order to free the general theory from inapplicable objections, it has been necessary to argue many points at such length that there may have been a certain sacrifice of perspective. In these last few pages, therefore, an attempt will be made to single out a few of the outstanding characteristics of that form of the theory of association which should be acceptable and useful in present-day psychology.

#### Associationism: A Genetic Theory

The modern theory of association is concerned with the fact that connections and organizations arise within an individual's activities as a result of the conditions under which the activities, themselves, occur. It is distinctly a genetic theory. Psychologists who are sympathetic with the general notion of association apply it even in the interpretation of perception, as is well illustrated by such a study as Strat-

ton's<sup>1</sup> on inverted vision. Perhaps the strong genetic bias of association theory stands out best when we compare it with that of gestalt. On numerous occasions the gestaltists have taken pains to attack the associational view because of its emphasis upon "past experience."

## Association Theory Is Merely a Scientific Theory

While association theory deals with certain phenomena occurring during individual development, it does not imply that all changes in behavior and mental life are associational in origin. James, who was an enthusiastic user of the conceptions of association and habit-formation, also believed that many features of human nature come forth largely as products of organic maturation. And there are plenty of associationists who recognize such other fundamental principles as those of adaptation, refractory phase, perseveration, conflict.

In other words, the theory of association is to be regarded simply as an attempt to organize a class of psychological phenomena. It is not necessarily, and to my mind should not be, a philosophy of mind. One need not deny that the conception of psychological association has had an influence upon epistemology. It is simply desirable that the psychological theory of association be sharply distinguished from those philosophical theories that make use of some of the same facts and terms. If this were done the theory of association would not be attacked on the grounds that it fails to take in all psychological phenomena. The only theory that could possibly do this in the present state of scientific knowledge would be one of those products of philosophy which tells all about everything in terms so general that it means almost exactly nothing at all.

Since the early days of British associationists, this theory

<sup>1</sup> *Psychological Review*, 1896, 3, 611-617, and 4, 341-360.

has been related to a generally empirical point of view. While there have been neurological speculators, like Hartley and Bain, the tendency has been to stress observable fact. This spirit may well be retained by modern thinkers. So far as observation goes, we have involved in associative connections sense-processes, perceptions, ideas, images, feelings, emotions, tendencies and sets, and bodily movements. There is no harm in speculative endeavors that aim to reduce all of such items to reflexes, to synaptic resistances, or to conditions of electrical potential in the brain. But such conjectures go beyond the relatively empirical level at which the general theory rests and we should recognize that the theory in its broad outlines does not stand or fall according to what neurological interpretation of association should ultimately prove tenable.

This distinction between the formulation of empirical principles and the indulgence in more speculative undertakings regarding neurological bases does not free the associationist from all physiological obligations. Despite the arguments of many philosophers, scientific psychology has never succeeded in isolating itself from physiology. Experimental psychology was actually born of physiology and the two have never been wholly separate during their history. The consequence of this would seem to be that the psychologist should never do actual violence to known physiological fact. He should not include under one type of association the connection between thinking of white and thinking of snow and the connection between the retinal incidence of a homogeneous light and the sense-process of seeing green.

Such a point of view, with its effort to be fair to the facts, whether they be of the kind called *conscious* or of the kind called *physiological*, cannot be tolerable to all minds. For many there can be no decent peace until all cats are gray. But whatever the emotional need for a monistic universe,

the experimental psychologist has never acted as though he lived in such a neighborhood.

So long as association theory remains at the scientific level, there are a number of profitless quarrels that can be avoided. It is sometimes said that this theory sees the reality of psychological states and activities only in the elemental states and activities into which the larger wholes can be broken. As a scientific procedure the theory does conceive of larger units of activity as made up of smaller units, but there is no reason why it should concern itself with the relative merits of parts and wholes. There are many possible logical bifurcations of the universe; mind and matter, particular and general, good and bad, truth and error, part and whole; but none of these sets an issue for science. It is sometimes said that the association theory cannot represent a valid type of scientific analysis, because it is unable to name the irreducible units that enter into associative connections. But here again we face a legitimate charge only if the association theory has ontological or epistemological aspirations.

### The Laws of Association

Throughout the history of associational analysis there has been a recurrent search for the one essential condition of associative connection. For some writers the *sine qua non* has been contiguity, for others similarity has been the great associative *It*. Recently we have been witnessing a new manifestation of this same tendency. Much effort has been put forth, both in experimentation and in argument, to show that association is *not* brought about by *mere* frequency or by *mere* contiguity. What a shame it is that these writers have stopped there! They could so easily have gone on to show that association is not brought about by *mere* anything.

If there be a single, general law of association, it is not to be identified with time-interval, the state of other connections, frequency of repetition, the intensity or satisfyingness of the activities involved. Rather the general law should be written as follows:

$$A = f(x, y, z \dots)$$

where

A is associative strength

x, y, z, etc. are such factors as time-interval, frequency of repetition, the state of other existing connections, sensory intensity, affective intensity, and other factors demonstrably related to associative strength

Thus, associative strength is a function of some combination of these factors.

Dunlap,<sup>2</sup> Thorndike,<sup>3</sup> Köhler,<sup>4</sup> as well as a few earlier writers,<sup>5</sup> have made much of the fact that high frequency values may be accompanied by low degrees of associative connection. As we have earlier shown (pp. 100, 102), this is only what is to be expected when exceptionally high frequencies bring us into contact with such effects as refractory phase and "fatigue." But it is also to be expected, upon occasion, providing the increase in frequency is not accompanied by a constancy in the other terms of our general equation. A given increase in frequency will give its expected increase in associative strength only if there does not occur concomitantly some more radical change in another variable, like affective tone, of such a nature that the benefits of increased

<sup>2</sup> "A Revision of the Fundamental Law of Habit Formation," *Science*, 1928, 67, 360-362.

<sup>3</sup> *Human Learning* (1931).

<sup>4</sup> *Gestalt Psychology* (1929), Chapter VIII.

<sup>5</sup> Eg. J. Peterson, "Frequency and Recency Factors in Maze Learning by White Rats," *Journal of Animal Behavior*, 1917, 7, 338-364, and Z. Y. Kuo, "The Nature of Unsuccessful Acts and Their Order of Elimination in Animal Learning," *Journal of Comparative Psychology*, 1922, 2, 1-27.

frequency are more than counterbalanced. It is my judgment that no such constancy has obtained in the experiments that have been cited in connection with this issue.

These recent proofs that association does not depend upon *mere* frequency may fairly be compared with the demonstration that shorter boards often have greater area than do longer boards. Therefore area is not determined by mere length. It would seem as though anyone who had clearly in mind the actual situation,  $\text{Area} = f(\text{length, breadth})$ , could hardly secure satisfaction from any such demonstration.

The special laws of association involve the detailed statement of the way in which any one of these factors is related to associative strength. In the case of the factors of time-interval and frequency it is possible to state the probable law graphically (see pp. 76, 103). A similar definiteness should be sought in connection with all of the factors that influence associative strength.

When the laws of association are looked upon in this way, it is seen that they set the program for innumerable experimental investigations. As Carr says:

“Further progress is impossible with the customary method of reflective analysis of ideational connections, for we are here dealing with highly complex conditions involving a multitude of indirect connections. The problem must first be experimentally attacked with simple behavior conditions; and the more complicated conditions must then be studied from the standpoint of their genetic development.”<sup>6</sup>

Many critics of the theory of association have not been able to see that this conception leads naturally from the library into the laboratory, though certainly the natural trend of events should have been made clear enough by the memory studies of Ebbinghaus, G. E. Müller, and their suc-

<sup>6</sup> “The Laws of Association,” *Psychological Review*, 1931, 38, 228.



cessors, and by the studies of animal learning from the level of conditioned salivation to that of maze running.

While the program laid out by modern association theory calls for experimentation and then more experimentation, we shall do well not to forget entirely the logical meticulousness of the earlier proponents of this conception. When too ready distinctions are made between *qualitative* and *quantitative* laws (p. 67), the mere logic of the situation may still be enough to bring about an altered point of view. When we face the current statement that learning is not always facilitated by increasing *frequency* and when elaborate experiments are brought forward to prove that this is the case, we should not lose sight of the fact that a half-hour of arm-chair cerebration would have indicated as much. Since there are such occurrences as work decrement and refractory phase and since these phenomena are positively related to frequency, learning could not possibly bear a one-way relation to that factor. When experiments on the influence of satisfi-  
ngness are brought forward, there is still a service for the disagreeable Socratic custom of asking for a definition of terms.

### Further Ramifications of Association Theory

It has been the purpose of the present essay to establish the fact that there is a tenable theory of association which is not a philosophy of mind, but which is a fruitful conception with definite indications for further experimentation and clearer definition. In the pursuit of this objective, it has been necessary to bring into consideration the application of the general viewpoint to an analysis of the conditions of association formation. But this does not mark the only application of the conception. There is an elaborate set of problems connected with the decay of associative connections

and we may state a general law of disintegration analogous to that of fixation. The effective factors will be disuse, character of interpolated learning, age of the connection, and the other recognized conditions of retention. Then, too, there are many interesting problems involving the relation of associative principles to other laws of psycho-dynamics. The question arises as to whether there is a principle of *perseveration* and what its relation may be to associative instigation. There are situations in the realm of sense perception where, as Helmholtz saw,<sup>7</sup> associative principles intrude into sensory physiology in an important but confusing manner.

But the extension of the theory to these other problems involves no important addition to the general viewpoint. Such an extension offers no present solution for many of the problems that will be met, but it does offer a method of thinking. And this method of thinking is marked by its effort to cast the old problems as well as the new into definite and experimentable form.

<sup>7</sup> See E. G. Boring, *A History of Experimental Psychology* (1929), pp. 296-309.

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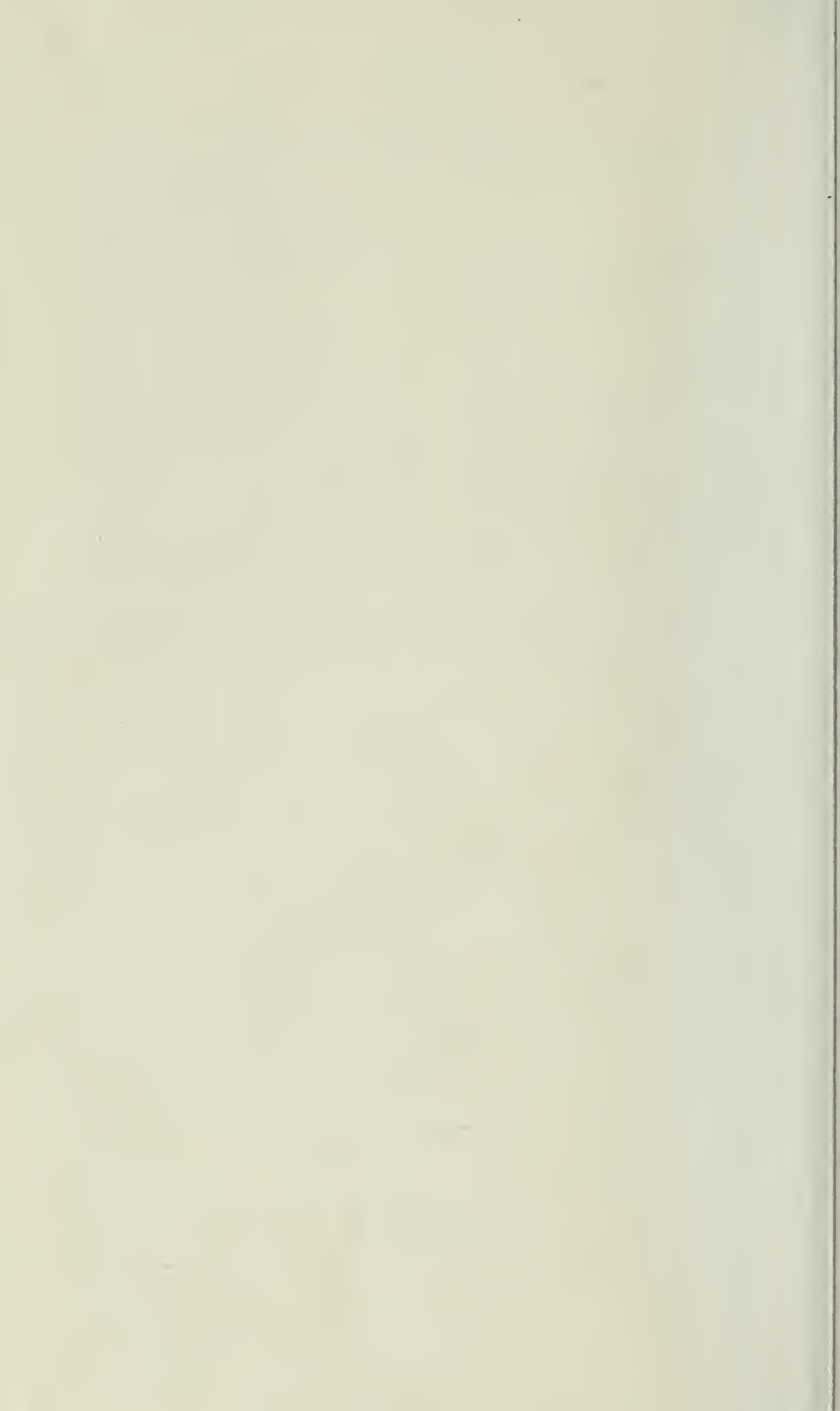
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